ESSAYS ON SUBJECTIVE WELL-BEING

A Ph.D. Dissertation

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To my parents and brother

ABSTRACT

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This dissertation consists of three essays on subjective well-being.

The first essay examines whether aggregate job satisfaction in a certain labor market environment can have an impact on individual-level job satisfaction. We seek an answer to this question using two different datasets from the United Kingdom characterizing two different labor market environments: Workplace Employment Relations Survey (WERS) at the workplace level (i.e., narrowly defined worker groups) and British Household Panel Survey (BHPS) at the local labor market level (i.e., larger worker groups defined in industry × region cells). Implementing an original empirical strategy to identify spillover effects, we find that one standard deviation increase in aggregate job satisfaction leads to a 0.42 standard deviation increase in individual-level job satisfaction at the workplace level and 0.15 standard deviation increase in individual-level job satisfaction at the local labor market level. These social interactions effects

are sizable and should not be ignored in assessing the effectiveness of the policies designed to improve job satisfaction.

Individuals tend to self-report higher subjective well-being levels on certain days of the weeks than they do on the remaining days, controlling for observed variation. The second essay tests whether this empirical observation suffers from selection bias by using the 2008 release of the British Household Panel Survey. In other words, we examine if subjective well-being is correlated with unobserved characteristics that lead the individuals to take the interview on specific days of the week. We focus on two distinct well-being measures: job satisfaction and happiness. We provide convincing evidence for both of these measures that the interviews are not randomly distributed across the days of the week. In other words, individuals with certain unobserved characteristics tend to take the interviews selectively. We conclude that a considerable part of the day-of-the-week patterns can be explained by a standard "non-random sorting on unobservables" argument rather than "mood fluctuations". This means that the day-of-the-week estimates reported in the literature are likely to be biased and should be treated cautiously.

In Sub-Saharan Africa, some scholars identify ethnicity as a cause of instability and poor economic growth, which is due to worse public policies. Eifert, Miguel, and Posner (2010) show that ethnic identification is more prominent during competitive election periods in comparison to other identifying categories such as gender, religion, and class/occupation. The third essay utilizes data from 12 Sub-Saharan African countries and over 40,000 respondents taken

from the Afrobarometer. It asks if individual subjective well-being changes in the run up to competitive elections. We find strong evidence that individual subjective well-being does change. It is positively related to the proximity to an election and this proximity effect depends on the competitiveness of the election. We further investigate the background mechanisms behind this positive relationship i.e.: to what extent does well-being of the individual change if the party that the individual supports wins the election, and is there a change in well-being of the individual before and after the election? In addition, we document that ethnic identification also has a positive impact on individual well-being after controlling for electoral cycle variables. Policy makers should internalize these positive externalities driven from politically-induced ethnic identification.

JEL codes: C25, C31, D60, D62, I31, J28, O55, O15.

Keywords: Subjective well-being; social interactions; spillovers; hierarchical model; day-of-the-week effects; self-selection; treatment effects; ethnicity; election; WERS; BHPS; Afrobarometer.

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However, while grateful to them, I bear the sole responsibilities for all the mistakes made in the thesis.

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CHAPTER 1

INTRODUCTION

For such a long time, economists have ignored researching individual subjective well-being. They left the study of happiness to other disciplines, especially psychology. When the science of economics was founded by the classics, they thought that happiness can be measured and used to determine whether a particular economic policy raises or lowers the happiness of the people affected. Thereby, Jeremy Bentham for instance assumed that utility reflects pleasures and pains, and Isidor Edgeworth believed that happiness can be measured by a hedonometer. The situation started to change with the normative economics that what the outcome of the public policy ought to be. It was followed by "New Welfare Economics", which was a normative branch of economics. John Hicks demonstrated that human behavior, and in particular, the demand for commodities can be explained on the basis of relative utility. Welfare judgements can be made by resorting to the Pareto criterion, and therefore, no comparison of welfare levels among individuals is required. This approach simplifies economic analysis.

However, today there is a dramatical change in economists' thinking. Psychologists have launched new ways of measuring utility [Kahneman, Diener, and Schwarz (1999)]. One way makes it possible to approximate individual utility in a satisfactory way by posing one question—or a combination of several question— in representative surveys. This question enables researchers to obtain indications of individuals' evaluations of their life satisfaction/happiness. In general, as in the literature, the terms "happiness", "well-being", and "life satisfaction" are used interchangeably.

Moreover, there is a vast literature that proves that the measures of reported subjective well-being can serve as a proxy for individual utility. From then onwards, there has been steadily increasing research in subjective well-being on the side of economists. Subjective well-being research adds a considerable number of new insights to well-known theoretical proportions. However, it is still debatable to what extent these "traditional" measures of subjective well-being accurately capture the various notions of individual utility. On the other hand, subjective well-being enables testing different aspects of life-events which could not be solved with using revealed preferences.

This dissertation consists of three essays on subjective well-being. The first and the third essay address the new possibilities offered by subjective well-being. The first one tries to understand whether individual subjective well-being can change with social interactions and the third one analyzes the relationship between personal identities such as ethnicity and political competition from a subjective well-being angle. The second chapter tests to what extend we can trust subjective well-being data given it's subject to "mood fluctuations".

More specifically, in order to analyze labor market phenomena, the first essay makes use of job satisfaction data with an interest on social interaction by utilizing the British Household Panel Survey Data (BHPS)[1] and the Workplace Employment Relations Survey (WERS)[2]. The second essay investigates whether the day-of-the-week effect estimates reported in the empirical subjective well-being literature suffer from selectivity bias in large panel dataset (BHPS). Finally, the last essay investigates subjective well-being in the framework of political and development economics. It examines the impact of competitive elections and ethnic identification on subjective well-being in Sub-Saharan Africa.

The following subsections give brief description about chapters, and outline the thesis.

1.1 Social Interactions in Job Satisfaction

In most research, job satisfaction is documented as being positively correlated with worker performance and productivity. But whether there are any visible footprints of social interactions in job satisfaction or not is an unanswered question. How do my colleagues job satisfaction levels in the workplace affect mine? To what extent does the job satisfaction of people with whom I am

[1]The BHPS provides information on individual, household, and job/employer-related characteristics from 1991 to 2008 in England, Scotland, Wales, and Northern Ireland.

[2]The WERS is a national survey of British employees constructed for the purpose of collecting information on employment relations in Britain.

working within the same industry and region influence my job satisfaction? What kind of group-level interactions can affect my job satisfaction level? Answers to these questions may be important for designing policies to increase job satisfaction. Overall, this study examines whether aggregate job satisfaction level in a certain labour market environment can have an impact on individual-level job satisfaction. If the answer is yes, then policies aimed to increase job satisfaction can increase productivity through social interactions as well.

This study conducts the analysis at two aggregation levels using two different datasets from the UK. First, the 2004 Workplace Employment Relations Survey (WERS) is to test the existence of job satisfaction spillovers at the workplace level. In the workplace-level analysis, the reference group that the social forces are effective is the set of workers in each workplace. Second, the 2004 British Household Panel Survey (BHPS) is to form industry-region cells for the purpose of testing the existence of spillovers at the local labour market level. The second exercise tries to capture social processes that involve collective aspects of community and work life. The conclusion of the paper is that there are sizable social interactions in job satisfaction that should not be ignored in assessing policy effectiveness.

An increase in aggregate job satisfaction level leads to three times higher increase in individual-level job satisfaction at the workplace than at the local labour market. Contextual social effects also have a significant impact on individual job satisfaction level. At the workplace level, job satisfaction at the individual-level goes up when a larger fraction of male and older workers are present. At the local labour market level, the findings say that individual-level job satisfaction score goes down as the fraction of workers with greater access to promotion opportunities goes up in each industry-region cell. There are significant income-comparison effects at the workplace, but not at the local labour market. In particular, individual-level job satisfaction goes down with average earnings and the fraction of high earners that is, those who earn above the median wage within the relevant worker population in the workplace.

These results suggest first, that there are large gains to policy interventions to increase individual-level job satisfaction, as there are significant positive feedback effects from group-level job satisfaction toward individual-level job satisfaction in the form of spillover externalities. Second, failing to account for the spillover externalities in job satisfaction may lead to an incorrect assessment of the effectiveness of job satisfaction policies. Thus, policy-makers should internalise these externalities. Third, job satisfaction spillovers are much stronger at the workplace level than local labour market level: therefore, designing and enforcing job satisfaction policies at the workplace level will likely be more effective than implementing such policies at the local labour market level.

1.2 Selection Correction on Day-Of-The-Week

Empirical studies document that individuals tend to report lower levels of happiness on Sundays and/or Mondays, whereas they tend to report higher job satisfaction levels on Fridays and/or Saturdays than the other days of the week, controlling for observed variation [Taylor (2006), Akay and Martinsson (2009), and Helliwell and Wang (2013)]. These results are based on the main micro-level datasets such as the British Household Panel Survey (BHPS), the German Socio-Economic Panel Survey (GSOEP), and the Gallup/Healthways polls as well as several small-scale surveys. This literature suggests that subjective well-being varies significantly across the days of the week.

Are people's mood really sensitive to the day-of-the-week or do people actually select the day-of-the-week that they take the survey depend on their daily routine and/or their unobserved characteristics? As an example, self reported job satisfaction is the highest on Fridays and Saturdays, it may be the case that hard-working individuals —already highly satisfied jobwise— have only time left to fill the survey on days like Friday or Saturdays since they are occupied with work during the week. Alternatively, individuals, who are not working hard throughout the week can prefer to take the survey on Sundays instead of resting or Monday, which can be a good reason for procrastination due to the overload go beginning of new week. These types of individuals can be already unsatisfied with their jobs or lives in general. Overall, if selectivity is in action, then this would weaken the argument of "mood" fluctuations over the days of the week.

To summarize, the main hypothesis we test in this essay is: the day-of-the-week estimates reported in the empirical literature may be contaminated with selection bias. Whether this hypothesis is rejected or not will be important for economic modeling. If the selection bias is significant and, as a result, the day-of-the-week effects disappear after selection correction, then this will cast doubt on the relevance of the "mood fluctuations" argument. Thus, the shadow hypothesis we test is the relevance of the "neoclassical stable preferences assumption" against preferences subject to "mood fluctuations".

This study finds significant positive selection both for job satisfaction and happiness, utilizing the 2008 release of British Household Panel Survey (BHPS). For job satisfaction, the ones who are interviewed on Fridays or Saturdays tend to report higher job satisfaction and for happiness, those who are interviewed on Sundays or Mondays tend to report lower happiness levels than a random sample drawn from the population of employed workers with a comparable set of observed characteristic would report. The conclusion of the paper is that the magnitude of the selection bias originating from these compositional shifts is so large that there is only little room for the "mood fluctuations" argument. After selection correction, the estimated treatment effects for job satisfaction are higher among males, non-married workers, workers with permanent jobs, public sector workers, workers in large firms, union members, workers with good health, workers who prefer to work less, workers with higher relative income, workers with higher education, and middle-aged workers. The patterns are similar for happiness as well.

These findings provide evidence that the existence of weekly cycles in individual subjective well-being may not be as relevant as the literature documents. There is a considerable individual-level unobserved heterogeneity determining well-being scores, and the compositional changes in interviewees in terms of these heterogeneous factors drive most of the observed differences is self-reported subjective well-being across the days of the week.

We do not totally rule out the state-dependent nature of utility. Utility may be changing across states if these states reflect some fundamental feature of individual utility; such as employment status, marital status, etc. We rather argue that day-to-day shifts in agents' valuation of economic objects do not have strong empirical basis, when selectivity is controlled for.

1.3 Competitive Elections and Ethnic Identification

Ethnic identity in Africa is formed the individual's life settings. The relevance of ethnic identification is controversial; some scholars argue that the source is culture, and for others is politics. Recently Eifert, Miguel, and Posner (2010) stated, the source of ethnic salience comes from political competition: in other words, proximity to competitive elections increases the strength of ethnic attachments.

In ethnically diverse countries, political parties have used ethnic identity to mobilize voters and to establish political alliances, leading in some cases to violent ethnic conflicts. In competitive elections this can result in the loss of lives and massive destruction of property. Easterly and Levine (1997)'s famous "growth tragedy" is primarily based on the strong link between ethnic heterogeneity and slow growth in Sub-Saharan Africa (SSA). Some scholars point out ethnicity as a cause of instability and poor economic growth, which is due to worse public policies.

This study poses the following questions "How does the competitive election affect happiness of the people?", "Does a competitive election make them happier than a landslide election?", "If a competitive election makes the people happier, what is the underlying mechanism?, and "How happy are the people if they identify themselves ethnically?" Answering these questions may be important for policies as opposed to politically-induced ethnic identification. Overall, the first aim of the study is to determine how competitive elections affect the individual subjective well-being when they are proximate. As the second aim, since competitive elections increase the salience of ethnic identification, this study investigates the relationship between ethnic identification and subjective well-being after controlling for electoral cycle variables.

We conduct the analysis with several well-being questions, which the Afrobarometer includes, across 12 African countries over 40,000 respondents. The results show that for every month closer a country is to a competitive election, on average individual-level subjective well-being has a 0.015 standard deviation increase. There are several possible mechanisms that account for this positive relationship. The more prominent ones are winning the election; subjective well-being of the individual gets positively stronger if the party that the individual support wins the competitive election, and the asymmetrical effects of the election; the proximity —before and after the election— is positively related to subjective well-being, but the impact before the election is greater than that after the election. The result of the second aim is that if individuals identify themselves ethnically, this is positively correlated with individual-level subjective well-being after controlling for electoral cycle variables.

These findings suggest that positive externalities exists from competitive elections and ethnic identification to the individual subjective well-being. These results should be taken into consideration when implementing policies opposed to politically induced ethnic identification. Ethnicity can help to develop society, both socially and economically, by mobilizing people to initiate development projects in their communities. However, one should be cautious about that these findings are only affected in short-term, the effects of long-term are unknown.

1.4 Thesis Outline

Chapter 2 examines whether there is any visible footprints of social interaction in job satisfaction by utilizing two different dataset and social environment. Chapter 3 first replicates the literature findings about relationship between subjective well-being and days-of-the-week and then asks if these day-of-the-week estimates for job satisfaction and happiness measures suffer from selection bias in large panel dataset. Chapter 4 investigates how competitive elections affect individual subjective well-being when they are proximate and how the ethnic identification is related to subjective well-being.

The first two chapters are co-authored with Semih Tumen. The companion work of Chapter 2 [Tumen and Zeydanli (2014)] is forthcoming at Journal of Happiness Studies. Chapter 3 is forthcoming at Social Indicators Research. The ideas contained in this thesis aim at contributing subjective well-being literature and shall not be associated with views of any of the aforementioned institutions nor their policies. Any errors are mine.

Each chapter is a self-contained manuscript. For the reader's convenience, the common bibliography is collected at the end of the thesis.

CHAPTER 2

SOCIAL INTERACTION IN JOB SATISFACTION

Job satisfaction is a direct measure of utility an employed worker derives from his/her current job [Clark and Oswald (1996)]. It has extensive behavioral consequences. For example, job satisfaction is a significant determinant of labor market mobility—in particular, the quitting behavior.[1] It is also shown to be related to relative pay comparisons among peer groups in the workplace.[2] Most importantly, and this is mainly why labor economists should be interested in job satisfaction, it is documented to have a positive correlation with labor productivity and worker performance.[3] In particular, Boeckerman and Ilmakunnas (2012) document that job satisfaction has a causal effect on productivity.[4] To get the feel of the magnitude, Boeckerman and Ilmakunnas

[1]See, e.g., Freeman (1978), Akerlof et al. (1988), Clark et al. (1998), and Clark (2001).

[2]See, for example, Clark et al. (2009) and Card et al. (2012).

[3]Other studies documenting this positive relationship include, but are not limited to, Iaffaldano and Muchinsky (1985), Ostroff (1992), Brown and Peterson (1994), Ryan et al. (1996), Sloane and Williams (2000), Argyle (2001), Judge et al. (2001), Harter et al. (2002), Schneider et al. (2003), Patterson et al. (2004), Green and Tsitsianis (2005), Otis and Pelletier (2005), Christen et al. (2006), Ghinetti (2007), and Wegge et al. (2007). Zelenski et al. (2008) and Oswald et al. (2014) argue that happiness and life satisfaction are also positively correlated with productivity.

[4]The direction of the causal relationship between productivity and job satisfaction has been controversial in the literature. However, recent evidence suggests that the direction of

(2012) find that one standard deviation increase in job satisfaction within the plant increases productivity per hours worked by 6.6 percent.

Although several aspects of job satisfaction have been studied extensively in the empirical literature, whether there exist spillover externalities in job satisfaction—i.e., whether individual-level job satisfaction is affected by the aggregate job satisfaction in a certain labor market environment—or not remains as an unanswered question. This is a relevant question because job satisfaction is often associated with workplace attitudes such as involvement in the organization, relatedness with co-workers/customers/managers, attachment, motivation, shirking, tendency to slow down work, absenteeism, etc. These attitudes form a workplace "atmosphere" and jointly contribute to the formation of worker satisfaction and performance. Therefore, the aggregate job satisfaction level in a certain work environment can be regarded as a "social" variable and may, in turn, affect individual-level job satisfaction.

Our ultimate goal in this paper is to investigate if there exist any visible footprints of social interactions in job satisfaction. Answering this question is also important for policy. If there exist positive spillovers in job satisfaction, then policies targeted to increase job satisfaction can boost productivity not only directly, but through spillover externalities too. When these social interactions effects are sizable, ignoring them may lead to mis-assessment of the effectiveness of the policies designed to improve job satisfaction in various work environments.

the relationship goes from job satisfaction to labor productivity.

We perform our empirical analysis at two aggregation levels using two different data sets from the United Kingdom. *First*, we use the Workplace Employment Relations Survey (WERS) to test the existence of job satisfaction spillovers at the workplace level (or establishment-level).[5] In the workplace-level analysis, the reference group that the social forces are effective is the set of workers in each workplace. *Second*, we use the British Household Panel Survey (BHPS) to form industry $\times$ region cells for the purpose of testing the existence of spillovers at the local labor market level. In this second exercise, we try to capture more general social effects in larger reference groups. The main purpose is to focus on social processes that involve collective aspects of community and work life. In both of these exercises, we concentrate on estimating the correlation between the group-level and individual-level job satisfaction scores, controlling for a large set of observed covariates. Drawing a distinction between the workplace and local labor market level analyses is useful, because it will allow us to make precise statements on whether it is more effective to enforce job satisfaction policies at the establishment level (i.e., as firm-specific policies) or local labor market level (i.e., in the form of broader institutional measures).

Our econometric framework will be a version of the canonical linear-in-means model, which is a base for the bulk of empirical work on social interactions.[6] The main problem with the linear-in-means model is that it necessitates employing a carefully-designed identification strategy to separate endogenous ef-

[5]The terms "workplace" and "establishment" will be used interchangeably throughout the paper.

[6]See Blume et al. (2011) for an in-depth background information on linear-in-means models, including a comprehensive discussion on micro-foundations and econometric identification. Also see Blume and Durlauf (2001), Brock and Durlauf (2001b), and Soetevent (2006).

fects from the contextual effects [Manski (1993)]. It will perhaps be useful at this point to clearly define the terms "endogenous social effects" and "contextual social effects."[7] The endogenous effect refers to the effect of the group-level outcome on the individual-level outcome. Within the context of our paper, this corresponds to the effect of the group-level mean of job satisfaction on the individual-level job satisfaction. The contextual effect, on the other hand, refers to the effect of the group-level counterparts of the individual-level observables on the individual-level outcome variable; in our paper, this corresponds to the effect of, say, group-level average age or average education on the individual-level job satisfaction score.

At the center of our identification strategy lies an insight from the hierarchical (or multilevel) statistical models of social processes: social groups describe "ecologies" in which decisions are made and matter because different ecologies induce different mappings from the individual determinants of these decisions to the associated outcomes [Raudenbush and Sampson (1999)]. Based on this conceptualization, we construct an empirical model in which contextual effects (i.e., the "ecologies" in our model) alter the coefficients linking individual characteristics to outcomes. This corresponds to allowing for multiplicative interactions between the contextual effects and the remaining explanatory terms within the linear-in-means model. We formally show that introducing these cross-product terms induces non-linearities that resolve the reflection problem Manski (1993) describes [see Section 2.3]. Such a setup secures the econometric identification of social interactions and enables us to separate endogenous

[7] See also Manski (2000) and Brock and Durlauf (2001b) for a more detailed discussion of the different types of social interactions effects.

effects from the contextual effects [Blume and Durlauf (2005)]. Although, this approach is rather simple and intuitive, it is surprisingly under-utilized in the literature.

There are two more potential threats to identification. The first one is the possibility of sorting into reference groups based on unobserved factors [Manski (1993)]. The sorting can be conducted two ways; one is via group-level unobserved factors and the other is via sorting into reference groups. These two can also be potentially related to each other. If group-level unobserved factors that determine individual-level job satisfaction exist and are also correlated with group-level job satisfaction, then the resulting estimates would be biased. Our empirical approach also allows us to address this problem, at least partially, by controlling for group-level unobservables in various ways. Endogenous sorting would occur in several dimensions. It may be the case that group-level unobserved factors trigger the selection but do not determine the individual-level satisfaction. This relationship can also work inversely, i.e., group-level unobserved factors may be more recent than the sorting. However, there are several limitations in solving endogenous group selection that we will discuss later. And, second, it is well-documented in the literature that the relative income structure within the reference group is an important determinant of the job satisfaction level in the peer group [Card et al. (2012)]. We also control for the pay-comparison effects in our calculations.

We find that one standard deviation increase in aggregate job satisfaction level leads to a 0.42 standard deviation increase in individual-level job satisfaction score at the workplace level and a 0.15 standard deviation increase in individual-level job satisfaction score at the local labor market level. In other words, we report that statistically significant job satisfaction spillovers exist both at the establishment level and local labor market level; and, the estimated spillovers are approximately three times larger at the establishment level than those at the local labor market level. These estimates can be restated in terms of the social multiplier: the corresponding social multipliers are [$1/(1 - 0.42)\approx$] 1.72 and [$1/(1 - 0.15)\approx$] 1.18 at the workplace and local labor market levels, respectively.[8] Simple calculations yield the result that the Boeckerman and Ilmakunnas (2012) estimates—which say that one standard deviation increase in job satisfaction within the plant increases productivity per hours worked by 6.6 percent—would be revised up to 11.4 percent at the workplace level and 7.8 percent at the local labor market level after accounting for the job satisfaction spillovers. To summarize, these results suggest that (1) failing to account for the spillover externalities in job satisfaction may lead us to mis-assess the effectiveness of job satisfaction policies; thus, the policy maker should internalize these externalities, and (2) job satisfaction spillovers are much stronger at the workplace level than local labor market level; therefore, designing/enforcing job satisfaction policies at the workplace level will likely be more effective than implementing such policies at the local labor market level.

We also report estimates for contextual social effects. At the workplace level, we find that individual-level job satisfaction goes up with the fraction of male and older workers in the workplace. At the local labor market level, however,

[8]See Glaeser et al. (2003) for an excellent discussion of the social multiplier concept.

gender and age do not have any statistically significant contextual effect; instead, we only find that individual-level job satisfaction score goes down as the fraction of workers with greater access to promotion opportunities goes up in each industry × region cell. We also document that there are significant "income comparison effects" at the workplace, but not at the local labor market. In particular, we find that individual-level job satisfaction goes down with (*i*) average earnings and (*ii*) fraction of high earners—i.e., those who earn above the median wage within the relevant worker population—in the workplace. We discuss these results further in Section 2.5, where we also present detailed robustness exercises to prove that our estimates are not excessively sensitive to relaxing some of the main assumptions behind the empirical model.

The plan of the paper is as follows. Section 2.1 relates and compares our work to the relevant papers in the literature. Section 2.2 explains the details of the econometric model and the identification strategy we employ. Section 2.3 provides an overview of the data sets we use and justifies the construction of our reference groups in different work settings. Section 2.4 presents the estimates, discusses in detail the results, performs robustness checks, and elaborates on the policy implications. Section 2.5 concludes.

2.1 Related Literature

Our paper can be placed into the literature investigating social interactions in labor markets. There is a large body of literature testing the existence of peer effects in various labor market outcomes including productivity, wages, absenteeism, and learning (or knowledge spillovers). The results are mixed. For example, using grocery scanner data from a large supermarket chain, Mas and Moretti (2009) perform a field experiment among low-wage earners to analyze if the individual-level effort is influenced by a permanent increase in the productivity of co-workers and find reasonably large peer effects. Falk and Ichino (2006) study the behavior of high school students performing a simple task in a laboratory experiment to understand if individual-level performances are directly affected by the existence of other individuals doing the same task and they also document moderate peer effects. Ichino and Maggi (2000) find that group-level peer absenteeism increases individual absenteeism. In a field study, Bandiera et al. (2009) find that individual-level productivity responds to the skill-level of a friend working nearby, but does not respond to the skill-level of a non-friend working around. Guryan et al. (2009), on the other hand, find employing a random assignment exercise on a golf tournament data that individual-level performance is not influenced by the playing partners' ability. Cornelissen et al. (2013) report only small peer effects in wages among co-workers.[9] While Azoulay et al. (2010) and Jackson and Bruegmann (2009) document significant knowledge spillovers among co-workers, Waldinger (2012) shows that those spillovers are weak, if they ever exist.

There are also several papers investigating contagion effects in subjective well-being measures. Using Chinese rural survey data, Knight and Gunatilaka (2009) examine whether happiness is infectious or not at the village level. Their results show that happiness is infectious in narrowly-defined reference

[9] Here, "peer effects in wages" refer to the idea that peer-induced productivity increases could be rewarded in the form of higher wages.

groups. They exploit the panel feature of their data set to remove the reflection problem and identify the relevant social effects. Papers in the psychology literature also find that happiness might be contagious in small environments.[10] However, these studies do not address the reflection problem, which might bias the results. Tumen and Zeydanli (2014), on the other hand, find that these contagion effects might disappear in more broadly defined reference groups.

Our paper differs from this body of work and contributes to the related literature in three ways. First, this is the first paper in the literature estimating spillover effects in job satisfaction. We show that there exist statistically and economically significant job satisfaction spillovers in various work environments. Second, we show that the degree of these spillover externalities may change at different aggregation levels. Using two different data sets from the United Kingdom, we construct our reference groups at two aggregation levels: workplace level and local labor market level. The former defines peer effects in narrowly defined work settings, while the latter defines the social environment in larger ecological settings that embed more general aspects of community and working life. We document that the job satisfaction spillovers exist in both environments; but, they are much stronger at the workplace level than local labor market level. We further argue that this may have important policy implications. And, third, motivated by the hierarchical models of social processes, we develop an original identification strategy to separate endogenous effects from the contextual effects, controlling for group-level unobserved heterogeneity.

[10] See, e.g., Hatfield et al. (1994), Sato and Yoshikawa (2007), and Fowler and Christakis (2008).

There are also several papers criticizing the empirical literature on peer effects.[11] In particular, Angrist (2014) has pointed out that many papers in the literature falsely interpret the observed correlations between individual- and group-level outcomes as causal relationships. He argues that the instrumental variable (IV) estimates reported in this literature "often ... produce findings that look like a peer effect, even in a world where behavioral influences between peers are absent." In this paper, we do not use the IV approach; instead, we rely on a non-linear model of social interactions—which is motivated by hierarchical statistical models—to obtain econometric identification. The non-linear models are not free of problems, either. The most common criticism is that, most of the time, the non-linear structure used to identify peer effects is hard to justify. The non-linear specification that we use in this paper has two appealing features. First, the non-linearity is simply obtained by including certain interaction terms between the regressors of a standard linear-in-means model. The use of interaction terms is quite common in regression analysis and are never regarded as strange or unjustified. Second, the inclusion of interaction terms into linear-in-means models is theoretically justified by the "social ecologies" viewpoint in a strand of the sociology literature.[12] See Raudenbush and Sampson (1999) and Blume and Durlauf (2005) for further motivation and references. Next, we present the details of our non-linear model of social interactions and describe our identification strategy.

[11]See, e.g., Moffitt (2001), and Angrist (2014).

[12]The origins of the term "social ecologies" or "ecological settings" goes back to the Chicago School of Sociology in the 1920s [see, e.g., Cavan (1983)].

2.2 Model and Theoretical Background

The econometric framework that we employ in this paper is a version of the canonical linear-in-means model of social interactions. Our ultimate goal is to estimate social interactions in job satisfaction. In particular, we would like to estimate the effects of (1) group-level job satisfaction—the "endogenous social effect"—and (2) group-level exogenous characteristics—the "contextual effects"—on individual-level job satisfaction, controlling for group-level heterogeneity. The linear-in-means model of social interactions is plagued with the well-known "reflection problem," which masks the econometric identification of social interactions [Manski (1993)]. The simplest way to resolve this issue is to use an appropriately formulated instrumental variables strategy. When an instrument is not available, it is necessary to invoke non-linearities to identify social interactions [Brock and Durlauf (2001a), Blume et al. (2011)].

In this paper, we use an empirical strategy that allows us to convert the standard linear model into a nonlinear one. The motivation comes from the hierarchical models of social processes. This hierarchical structure secures identification of social interactions via introducing cross-product terms into the standard model. This section provides a detailed description of our econometric model for the purpose of familiarizing the reader with the basic concepts we frequently mention throughout the paper. Section 2.2.1 presents our empirical model and the associated technical issues (i.e., the reflection problem) including a formal statement of the conditions required to identify social interactions. Section 2.2.2 describes our hierarchical model and assesses in detail how we achieve identification.

2.2.1 The Empirical Model of Social Interactions

Each individual $i \in \mathcal{I}$ is a member of a group $g \in \mathcal{G}$, where $\mathcal{I}$ is the number of individuals in the worker population and $\mathcal{G}$ is the number of groups, with $\mathcal{I} > \mathcal{G}$. The following linear-in-means equation is an empirical tool commonly used in the literature:

$$\omega_{i_g} = \beta_0 + \beta_1 \boldsymbol{X}_{i_g} + \beta_2 \boldsymbol{Y}_g + J m_g + u_g + \epsilon_{i_g}, \tag{2.1}$$

where the dependent variable, ω_{i_g}, is the individual-level job satisfaction for person i in group g, $\boldsymbol{X}_{i_g}$ is a vector of individual-level observed characteristics of i in group g, $\boldsymbol{Y}_g$ is a vector of group-level observed characteristics of group g, $m_g = \mathbb{E}[\omega_{i_g}|g, F_{i_g}]$ is the mean job satisfaction in group g, u_g is a group-level unobserved factor common across the members of group g, and ϵ_{i_g} is a random error term with $\mathbb{E}[\epsilon_{i_g}|g, F_{i_g}] = 0$. In our notation, F_{i_g} corresponds to the empirical distribution of individuals in group g and this distribution is possibly different for each group. The distinction between β_2 (contextual effects) and J (endogenous effect) is the key notion in this model. The former measures the effect of exogenous group-level variables on the individual-level outcome, while the latter measures the effect of endogenous group-level outcome on the individual-level outcome. Our ultimate goal is to clearly distinguish β_2 from J and to separately identify the effects of group-level variables on the individual-level outcome. However, econometric identification is a problematic issue in this standard setting. In what follows, we shut down the group-level unobserved effect u_g for notational simplicity. It will reappear in our final equation.

To define the identification problem, we take the conditional mathematical expectations of both sides of the linear-in-means equation, where the conditioning is on g and F_{i_g}, for all i and g. This gives us

$$m_g = \beta_0 + \beta_1 \boldsymbol{X}_g + \beta_2 \boldsymbol{Y}_g + J m_g, \tag{2.2}$$

where $\boldsymbol{X}_g = \mathbb{E}[\boldsymbol{X}_{i_g} | g, F_{i_g}]$. $\boldsymbol{X}_g$ can be named as the group-level mean of individual-level observed characteristics and it may or may not coincide with $\boldsymbol{Y}_g$. Notice that m_g appears in both sides of Equation (2.2). Solving for m_g yields the result that

$$m_g = \frac{\beta_0}{1-J} + \frac{\beta_1}{1-J}\boldsymbol{X}_g + \frac{\beta_2}{1-J}\boldsymbol{Y}_g. \tag{2.3}$$

The reflection problem states that if the dimensions of the vectors $\boldsymbol{X}_g$ and $\boldsymbol{Y}_g$ are the same, then linearity masks the econometric identification of the (endogenous) social interactions parameter J.

To formalize this statement, we plug Equation (2.3) into Equation (2.1), which gives us the estimating equation

$$\omega_{i_g} = \frac{\beta_0}{1-J} + \beta_1 \boldsymbol{X}_{i_g} + \frac{J\beta_1}{1-J}\boldsymbol{X}_g + \frac{\beta_2}{1-J}\boldsymbol{Y}_g + \epsilon_{i_g}. \tag{2.4}$$

When the reflection problem is in effect, J and β_2 cannot be distinguished from each other, which implies that social interactions cannot be identified. To see this, set $\boldsymbol{X}_g = \boldsymbol{Y}_g$, which yields the equation

$$\omega_{i_g} = \frac{\beta_0}{1-J} + \beta_1 \boldsymbol{X}_{i_g} + \frac{J\beta_1 + \beta_2}{1-J} \mathbf{Y}_g + \epsilon_{i_g}. \tag{2.5}$$

It is obvious that, in this equation, it is impossible to separate J from β_2 econometrically. One solution is the existence of an additional X_g which is not in $\boldsymbol{Y}_g$. If such an X_g exists, then endogenous social interactions (J)—and also all the other model parameters—are identified by applying simple ordinary least-squares method on Equation (2.4). In other words, one individual-level variable, the mean of which cannot be regarded as a group-level variable, is required for identification of social interactions.

Unfortunately, most of the large data sets—such as BHPS, GSOEP, WERS, etc.—do not include a variable X_g that can naturally fit into the IV definition provided above. One popular alternative to IV is to introduce non-linearities into the linear-in-means specification. To demonstrate how non-linearities secure identification, we modify the standard model as follows:

$$\omega_{i_g} = \beta_0 + \beta_1 \boldsymbol{X}_{i_g} + \beta_2 \mathbf{Y}_g + J\phi(m_g) + \epsilon_{i_g}, \tag{2.6}$$

where $\phi(\cdot)$ has non-zero second derivatives; that is, it is a legitimate nonlinear function. Without loss of generality, we assume also that $\phi(\cdot)$ is invertible. Again, taking the conditional mathematical expectations of both sides and rearranging the terms in such a way that the terms with m_g appears on the left and the rest of the variables on the right, we get the equation

$$\Phi(m_g) = \beta_0 + \beta_1 \boldsymbol{X}_g + \beta_2 \boldsymbol{Y}_g, \tag{2.7}$$

where $\Phi(m_g) = m_g - J\phi(m_g)$. The functions $\phi(\cdot)$ and $\Phi(\cdot)$ has the same properties, therefore we can invert $\Phi(\cdot)$ to get

$$m_g = \Phi^{-1}\left(\beta_0 + \beta_1 \boldsymbol{X}_g + \beta_2 \boldsymbol{Y}_g\right) \tag{2.8}$$

and plugging this into the original estimating equation we get

$$\omega_{i_g} = \beta_0 + \beta_1 \boldsymbol{X}_{i_g} + \beta_2 \boldsymbol{Y}_g + J\phi\left[\Phi^{-1}\left(\beta_0 + \beta_1 \boldsymbol{X}_g + \beta_2 \boldsymbol{Y}_g\right)\right] + \epsilon_{i_g}. \tag{2.9}$$

In such a setting, we can identify β_2 and J separately without a further need for an exclusion restriction (or an IV). One problem with this framework is that there is no systematic way of choosing the functional form of $\phi(\cdot)$. In this paper, we propose an estimation strategy that introduces a systematic way to embed non-linearities into the standard empirical specification. To be specific, we construct a hierarchical model, which has the additional advantage of being consistent with our definition and conceptualization of social interactions in job satisfaction.

2.2.2 The Hierarchical Model

Suppose that the Equation (2.1) is modified as follows:

$$\omega_{i_g} = \alpha_0 \left(\boldsymbol{Y}_g\right) + \alpha_1 \left(\boldsymbol{Y}_g\right) \boldsymbol{X}_{i_g} + \alpha_J \left(\boldsymbol{Y}_g\right) m_g + u_g + \epsilon_{i_g}. \tag{2.10}$$

In words, the coefficients α_0, α_1, and α_J are stated as functions of the contextual variables, $\boldsymbol{Y}_g$, which define the "social context." In other words, the contextual variables describe the properties of the environments that the individuals live in. Setting up the regression coefficients in this way implies that social groups describe ecologies in which decisions are made and matter because different ecologies induce different mappings from the individual determinants of these behaviors and choices.[13] To convert this setting into an empirical equation that we can estimate, we make the following simplifying assumptions:

$$\alpha_0 \left(\boldsymbol{Y}_g\right) = \beta_0 + \beta_2 \boldsymbol{Y}_g,$$

$$\alpha_1 \left(\boldsymbol{Y}_g\right) = \beta_1 + \boldsymbol{b}\boldsymbol{Y_g},$$

$$\alpha_J \left(\boldsymbol{Y}_g\right) = J + \pi \boldsymbol{Y_g}.$$

[13] As an example, suppose that we have two contextual variables in a workplace environment: age and gender. In such a case, whether the environment is dominated by young versus old workers or female versus male workers or not really matters. In other words, the determinants of job satisfaction will depend on the "atmosphere" formed by these team or group attitudes.

Plugging these expressions into Equation (2.10) yields

$$\omega_{i_g} = \beta_0 + \beta_1 \boldsymbol{X}_{i_g} + \beta_2 \boldsymbol{Y}_g + J m_g + \pi \boldsymbol{Y_g} m_g + \boldsymbol{Y}_g' \boldsymbol{B} \boldsymbol{X}_{i_g} + u_g + \epsilon_{i_g}, \tag{2.11}$$

where $\boldsymbol{B}$ is the matrix form of the coefficient vector $\boldsymbol{b}$. This equation looks very similar to our original linear-in-means specification except that we include interaction terms in the form of cross products motivated by the hierarchical model.

Note that the nature of the unobserved group-level effect u_g is a crucial issue. There are two alternatives: random effects versus fixed effects. If the true unobserved group-level effect can be controlled for up to a random error term, then a common way to resolve this issue is to assume that u_g is itself random rather than fixed. Assuming that u_g is random is equivalent to saying that it is uncorrelated with the regressors. However, group-level unobserved factors can easily be argued to be correlated with, say, group-level job satisfaction. When this is the case, not being able to control group fixed effects will yield erroneous results. In our baseline analysis, we assume that u_g is a random term. We then relax this assumption and control for group-level fixed effects to check if the results differ [see Section 2.4]. We also cluster standard errors at the group level, which means that we account for within-group correlations in the error structure.

To demonstrate how this model is identified, we take the conditional mathematical expectations of both sides, as before, and solve the resulting equation for m_g, which gives us

$$m_g = \frac{\beta_0 + \beta_1 \boldsymbol{X}_g + \beta_2 \boldsymbol{Y}_g + \boldsymbol{Y}_g' \boldsymbol{B} \boldsymbol{X}_g}{1 - J - \pi \boldsymbol{Y}_g}. \tag{2.12}$$

Notice that, very similar to the motivation behind the nonlinear model, this model also introduces non-linearity between m_g and the other regressors, even when $\boldsymbol{X}_g = \boldsymbol{Y}_g$. There is no need for an exclusion restriction and econometric identification of social influences is immediate, given standard conditions on individual- and group-level observed covariates [see Blume and Durlauf (2005) for further details]. This formulation is consistent with our hypothesis and our definition of social interactions.

At the end, we estimate Equation (2.11) to separately identify β_2 and J. In this setup, the endogenous social effect is $J + \pi \bar{\boldsymbol{Y}}_{\boldsymbol{g}}$, where $\bar{\boldsymbol{Y}}_{\boldsymbol{g}}$ are the sample means of group-level variables, i.e., the endogenous effect is no more J since we have cross-product terms in the regressions. The same logic applies to the contextual effects we estimate. The estimates we report and discuss in Section 2.4 directly refer to these "marginal effects."

2.3 Data and Reference Groups

In this section, we provide a detailed description of the two data sets we use in our empirical analysis: Workplace Employment Relations Survey and British Household Panel Survey. Both of these surveys are nationally representative data sets for the United Kingdom and provide establishment-level and individual-level labor market information, respectively. We also describe in detail the construction of our reference groups for both of these data sets. We focus on the 2004 editions of both data sets.

2.3.1 Workplace Employment Relations Survey (WERS)

WERS is a national survey of British employees constructed for the purpose of collecting information on employment relations in Britain.[14] The survey provides information about workers, working conditions, and industrial relations from all sectors except primary industries and private households with domestic staff. WERS 2004—the version that we use in our analysis—is the fifth among a series of surveys. Previous surveys are conducted in 1980, 1984, 1990, and 1998. In the 2004 cross-section, there are around 2,300 workplaces, 1,000 employee representatives, and 22,500 employees.

We construct the job satisfaction scores using the following seven question in the WERS-2004 data set. How satisfied are you with

1) the sense of achievement you get from work?

2) the scope for using your own initiative?

3) the amount of influence you have over the job?

4) the training you receive?

5) the amount of pay you receive?

6) the job security?

7) the work itself?

[14] The most recent version of this data set has been co-sponsored by the Department for Business, Innovation and Skills (BIS), Acas, the Economic and Social Research Council (ESRC), the UK Commission for Employment and Skills (UKCES), and the National Institute of Economic and Social Research (NIESR).

The responses are based on a five-point scale with 1 representing "very satisfied," 2 "satisfied," 3 "neither satisfied nor dissatisfied," 4 "dissatisfied," and 5 "very dissatisfied." For each of the seven questions listed above, we construct a binary variable for the positive responses—taking the value 1 for the "very satisfied" or "satisfied" responses and 0 otherwise—and, then, we construct a sum of the seven binary variables for each individual to form an index with values from 0 to 7 [see also Jones et al. (2009), Jones and Sloane (2010), and Mumford and Smith (2013)].[15] We use this aggregate measure as the "job satisfaction score" in our analysis. The average job satisfaction score in our sample is 4.20 and the standard deviation is 2.13. The BHPS data set, which we describe in the following subsection, has a 1–7 scale constructed based on different principles. For the sake of comparability of the estimates, we standardize the main job satisfaction measures in both WERS and BHPS around zero mean and unit variance. Thus, the dependent variable in our analysis will be the "standardized job satisfaction."

We control for a large set of individual- and job-related characteristics. To achieve consistency between the two data sets, we construct the WERS variables similar to their counterparts in the BHPS data set. After excluding missing information on our control variables and dropping workplaces with less than two employees, the WERS data set includes 1,673 workplaces/establishments and, in each workplace, up to 25 randomly-chosen employees taking the ques-

[15] Although Mumford and Smith (2013) use the six facets of job satisfaction in the WERS, neglecting the training, Jones and Sloane (2010) use all of them. Jones et al. (2009) argue that training is also an important component for job satisfaction. We also include training.

tionnaire.[16] We start with describing the education variables. Since this is a workplace-level data set, "No Qualification" category includes only a very small number of observations; thus, we drop the observations in this category and concentrate on the following education levels: "Higher Degree" (refers to postgraduate education), "First Degree" (refers to college education), "A-level," "O-level" (both referring to different classes of high-school education), and "Vocational Qualification."[17] Earnings variable in the WERS is reported in 14 pre-specified intervals,[18] and, following Mumford and Smith (2009), we use the midpoints of these intervals as our earnings variable for each individual. The last interval is open ended, so it does not have a midpoint; instead, we use the mean earnings for the last interval. In our sample, the average weekly log earnings is around 5.7. We also include relative earnings as a dummy variable taking 1 if the employee earns more than the mean earnings in the sample. We categorize the job status under three sector categories: private sector job, public sector job, and other. An establishment size variable is generated from the question of "Currently, how many employees do you have on the payroll at this establishment?" The answer varies from 5 to 10,000. We construct three variables for establishment size; small establishment (less than 50 employees), medium-size establishment (between 50 and 200 employees), and large establishment (more than 200 employees). Working hours are simply represented

[16]For consistency, the individuals in the BHPS data set who work in firms with less than two employees are correspondingly dropped.

[17]To be concrete, after constructing our sample, we observed that only two individuals remained in the data with no school degrees. We think that dropping these two observations would be more reasonable than generating a new education category called "no degree." Such a category, on the other hand, exists in our BHPS sample since the number of individuals with no degree is non-negligible in the BHPS data.

[18]The question of the earnings variable is the following: "How much do you get paid for your job here, before tax and other deductions?"

as a dummy variable taking 1 if the actual hours worked is above the sample mean and 0 otherwise.

Among 20,035 employees and 1,673 establishments in our sample, the average age is 42, 47 percent are male, and 68 percent are married. Higher degree has the lowest fraction, whereas vocational qualifications have the highest. 46 percent of the employees are union members. 55 percent of the workplaces are publicly owned. Regarding the establishment size, the shares of small, medium, and large establishments are 0.32, 0.32, and 0.36, respectively. See Table (2.1) for detailed summary statistics for our WERS sample.

2.3.2 British Household Panel Survey (BHPS)

The BHPS provides information on individual-, household-, and job/employer-related characteristics from 1991 to 2008 in England, Scotland, Wales, and Northern Ireland. It yearly follows the same representative sample of households interviewing every adult member of sampled households. Eighteen waves of data are available. To make the two data sets comparable and compatible, we focus on the 2004 cross-section of the BHPS.

The individual-level job satisfaction in the BHPS data set is reported based on a seven-point scale ranging from 1 (not satisfied at all) to 7 (completely satisfied). The employed workers are asked to rate the job satisfaction levels regarding the total income, job security, the actual work itself, and hours worked. The last question about job satisfaction is "Overall, how satisfied or dissatisfied are you with your present job?", which is again measured on the 1–7 scale and used as the "job satisfaction score" in our analysis. As we explain above, we standardize the job satisfaction score around zero mean and unit variance to achieve consistency across the job satisfaction measures we use for the WERS and BHPS data sets.

For the individual-level observed characteristics, we control for gender, age, education level, marital status, earnings, and pay comparisons. We collapse the education-levels into seven broad groups as follows: *higher degree* refers to postgraduate education, *first degree* refers to college education, *A-level*, *O-level*, and *other higher qualification* refer to high school graduates of different types (consistent with the education system in the UK), *vocational qualification* refers to teaching, nursing, commercial, apprenticeship, and the certificate of secondary education (CSE), and, finally, the ones with *no qualification*. The earnings variable—usual gross pay per month: current job—is recorded as the actual amount received and, thus, we simply take the natural logarithm of this variable in our analysis. We also consider the "taste for working hours" variable. Promotion opportunities is described by the binary variable taking 1 if the worker has access to promotion opportunities and 0 otherwise. The rest of the variables—firm size, job status, relative earnings, and union membership—are constructed similar to their counterparts in our WERS sample.

Table (2.2) presents the summary statistics of the sample that we use in our analysis. In order to be included into our sample, the respondent have to be employed and report a job satisfaction score. The mean age of the respondents is 40.4. Among the 6,428 observations, 47.4 percent are male, 57.3 percent are married, 4.4 percent have higher degree, 15.8 percent have first degree, another 12.6 percent have A-level degree, 17.6 percent have O-level degree, 30.8 percent have other higher qualifications, 9.4 percent have vocational qualifications, and the remaining 9.4 percent have no qualifications. Before standardization, the mean job satisfaction score is approximately 5.4 out of 7, with a standard deviation of 1.26. 79 percent are employed in full-time jobs. 63 percent are employed in privately-owned firms. 32.8 percent prefer to work fewer hours. 48.6 percent are employed in small-size firms. See Table (2.2) for further information on region- and industry-specific details. We generate group-level variables based on our reference groups constructed as industry $\times$ region cells. Below we describe how we construct our reference groups both in the WERS and BHPS data sets.

2.3.3 Reference Groups

Our primary objective is to separately identify endogenous social effects and contextual social effects in job satisfaction within a formal empirical model of social interactions. We conceptualize the social interactions that we estimate as the existence of "spillovers" in the society in the sense that the group-level job satisfaction in one's reference group affects the individual worker's perception of own job satisfaction. We perform this task at two levels with two different data sets from the United Kingdom. First, we use the WERS data set to estimate spillovers at the workplace level. And, second, we use the BHPS data set to estimate job satisfaction spillovers at the local labor market level. The WERS data set captures the social effects among co-workers, who are directly interacting. The BHPS data set, on the other hand, captures social effects among individuals who are potentially interacting indirectly. As Bramoulle et al. (2009) clearly state, this type of social effects is based on the idea that "neighbors in the neighborhood do not affect me directly; what matters is the neighborhood itself."

WERS. The WERS data sets naturally offers establishment-level reference groups; that is, all workers employed in a given establishment constitute the reference group for each of the workers employed in that establishment. There are 1,673 establishments in our WERS sample. Thus, the number of reference groups is 1,673. The average group size is approximately 12 worker per establishment. This setting defines narrow reference groups hypothesizing that social forces operate at the workplace level: workers in a given establishment are exposed to similar work-specific conditions that shape their job satisfaction perceptions. The group-level counterparts of the individual-level variables are constructed taking averages at the workplace level. Similarly, the endogenous social variable (the group-level job satisfaction score) is calculated by averaging the job satisfaction scores within the workplace.

BHPS. For the BHPS data set, we construct industry $\times$ region cells as our reference groups. In terms of our conceptualization of social interactions, this means that we try to capture the social forces that operate among workers who are geographically close to each other and who are potentially exposed to similar local labor market conditions specific to the industries they belong to. This is a common way of constructing reference groups in the empirical social interactions studies, particularly the ones handling large data sets. For example, Luttmer (2005) utilizes the outgoing rotation groups feature of the Current Population Survey and constructs industry $\times$ occupation cells to estimate the neighborhood effects of income on individual-level happiness. Similarly, Ferrer-i-Carbonell (2005) uses the German Socio-Economic Panel and constructs education $\times$ age $\times$ region cells to estimate the impact of the group-level income on individual-level subjective well-being. In a similar context, Glaeser et al. (1996) construct region-specific cells on a lattice to estimate the impact of neighbors' criminal-activity decisions on the agent's own decision to participate in crime. In another example, Stutzer and Lalive (2004) use data from Switzerland cantons and construct canton-level cells to estimate the effect of social norm to work—roughly, the rate of employment in one's neighborhood—on how quickly the unemployed individual finds a job, probably due to social pressure. The examples can be extended further. In all of these papers, large reference groups are constructed to capture the peer influences in broad social settings.

In our BHPS sample, the following twelve regions describe the geographical clustering: 1) London, 2) South East, 3) South West, 4) East Anglia, 5) East Midlands, 6) West Midlands, 7) North West, 8) North East, 9) Yorkshire & Humberside, 10) Wales, 11) Scotland, and 12) Northern Ireland. Nine industry categories are selected at one-digit level as follows: 1) energy & water supplies, 2) extraction of minerals & manufacture of metal goods, mineral products & chemicals, 3) metal goods, engineering & vehicles, 4) other manufacturing industries, 5) construction, 6) distribution, hotels & catering (repairs), 7) transport & communication, 8) banking, finance, insurance, business services & leasing, and 9) other services. At the end, there are 108 reference groups in our BHPS sample. The average group size is approximately 60 workers per industry $\times$ region cell.

2.4 Results and Discussion

In this section, we present the estimation results, provide a detailed interpretation of the estimates, perform robustness checks, and discuss the policy implications. We use two data sets: WERS and BHPS. In WERS, establishments are the reference groups, whereas, in BHPS, reference groups are defined by the industry $\times$ region cells. We group our estimates under three categories: individual-level coefficients, endogenous social effects, and contextual social effects. Individual-level coefficients describe the impact of individual-level observed covariates on the job satisfaction score. The endogenous social effect refers to the effect of the mean job satisfaction—where the mean is calculated at the group level—on the job satisfaction score. The contextual social effect refers to the effect of group-level counterparts of the individual-level covariates on the job satisfaction score. Below we discuss our estimates in detail. Note that both the individual- and group-level job satisfaction scores are standardized around mean zero and unit variance.

It will perhaps be useful to clearly explain how we present the results. We start with a model, which we call the "baseline" case, relying on two strong assumptions: (i) group-level means of the key variables are calculated by including the individual's own outcomes and/or characteristics and (*ii*) group-level fixed effects are omitted. We acknowledge that these are strong assumptions and can contaminate the estimates. We then proceed by relaxing each of these assumptions. The final results will be the most realistic ones. This line of reasoning will allow us to evaluate the potential biases that would be caused by each of these assumptions.

2.4.1 Hierarchical Model

We start our analysis by estimating our core model: the hierarchical model of social interactions given by Equation (2.11). The estimations are performed both for the WERS & BHPS data sets. In the baseline specification, we control for group-level effects by defining them as unobserved random effects. We relax this assumption in Section 2.4.2, where we perform robustness checks. We report the estimates in two forms. First, we document only the marginal effects for WERS and BHPS given in Tables (2.4) and (2.5), respectively. The marginal effects are readily interpretable as the social interactions estimates, so they will be at the center of our analysis. Second, we deal with the interaction terms. The coefficients of the interaction terms are useful, because they inform us whether the social interactions effects are heterogeneous or not.

2.4.1.1 Marginal Effects

Below we separately report the marginal effects for individual-level coefficients, endogenous social effects, and contextual social effects. As usual, the marginal effects evaluate the interaction terms at their respective sample means.

Estimates for the Individual-level Coefficients. Our estimates for the individual-level coefficients are parallel to those reported in the previous empirical literature on the determinants of job satisfaction [see, for example, Clark (1996), Clark and Oswald (1996), and Taylor (2006)]. Specifically, for both WERS and BHPS, we find that females, married workers, younger workers, workers with higher earnings, workers earning more than the median wage earner in the population, workers with greater access to promotion opportunities, and workers employed in smaller establishments are more satisfied jobwise.[19] The first two columns of Tables (2.4) and (2.5) report the estimates of individual-level covariates for the WERS and BHPS data sets, respectively. This paper focuses on estimating spillovers in job satisfaction; thus, the rest of the paper aims to interpret the estimated social effects rather than providing a lengthy discussion of the individual-level covariates.

Estimates for Endogenous Social Interactions. A group-level variable is endogenous if its individual-level counterpart is the choice variable of interest. Hence, the associated group-level variable can be defined as the effect of other people's behavior on individual-level behavior. This a classic example of spillover externalities. The findings from our benchmark estimates verify that there exist significant positive spillover externalities in job satisfaction; that is, the group-level (i.e., mean) job satisfaction is positively related to individual-level job satisfaction. To put it differently, an individual worker's job satisfaction level tend to be higher in a group of workers who are highly satisfied

[19] Note that the estimates for the promotion opportunities are only relevant for the BHPS, since the WERS data set does not include a question regarding the promotion prospects of the employees.

jobwise. We document these effects for both the WERS and BHPS samples. We find that one standard deviation increase in aggregate job satisfaction level leads to a 0.42 standard deviation increase in individual-level job satisfaction at the workplace level and 0.15 standard deviation increase in individual-level job satisfaction at the local labor market level. The first line of the estimates both in Tables (2.4) and (2.5) reports the numbers for endogenous spillovers for the WERS and BHPS data sets, respectively. Job satisfaction is often associated with workplace attitudes such as involvement in the organization, relatedness with co-workers/customers/managers, attachment, motivation, shirking, tendency to slow down work, absenteeism, etc. These attitudes form a workplace "atmosphere" and jointly contribute to the formation of worker satisfaction and performance. Our estimates confirm that the aggregate job satisfaction level in a certain work environment can be regarded as a "social" variable and may, in turn, affect individual-level job satisfaction significantly.

This result suggests that there are huge gains to policy interventions to increase individual-level job satisfaction as there are large positive feedback effects from group-level job satisfaction toward individual-level job satisfaction in the form of spillover externalities.[20] The degree of this feedback is larger at the workplace level than local labor market level. Thus, enforcing job satisfaction policies at the workplace level will likely be more effective than implementing such policies at the local labor market level. This result is particularly important, because it is reported in the literature that job satisfaction is positively

[20] Employers can stimulate social interactions among workers, which suggests that the optimal design of worker groups/teams should also account for these social forces [Tumen (2012)].

related to worker productivity [see, for example, Boeckerman and Ilmakunnas (2012)]. In terms of the magnitudes, Boeckerman and Ilmakunnas (2012) report that one standard deviation increase in group-level job satisfaction raises productivity per hours worked by 6.6 percent. This means that the existence of spillover externalities introduces notable gains to increasing job satisfaction at the individual level.

Estimates for the Contextual Effects. We control for a large set of contextual variables in our regressions. However, only a few of them produce statistically significant coefficients. We start with the WERS results, in which we report estimates for social interactions at the workplace level. Our WERS regressions [see Table (2.4)] show that the Male, Age, Log Earnings, and Relative Earnings variables are subject to statistically significant contextual social effects. To begin with, we show that working close to a group of workers with a larger fraction of males increases job satisfaction at the workplace-level analysis. This result can be interpreted in several ways. It is well-documented in the literature that females are more likely to be absent from work due to illness-related reasons.[21] If this is the case, workplace attitudes such as motivation, attachment, and involvement might be weaker for females than males due to these relatively more frequent breaks in their work attendance. As a consequence, working in a group with a greater fraction of males might increase motivation and, thus, job satisfaction. A second explanation might be related to gender discrimination; that is, our finding can be interpreted as the existence of distaste against women. However, we are cautious on this inter-

[21] Ichino and Moretti (2009) show that this may be related to menstrual cycles.

pretation as we do not have additional empirical support for this claim in our analysis. Apart from the contextual gender effects, we document that job satisfaction is higher in groups with higher average worker age and this positive impact becomes weaker as the average age goes up in our WERS sample. This can be attributed—using the Mincerian language—to labor market experience. Working in a group with a larger fraction of experienced workers may produce external effects boosting job satisfaction and, thus, worker productivity.

We also find that earnings have statistically significant contextual effects in our WERS regressions. The contextual earnings effect refers to the effect of the mean earnings in one's reference group on individual-level job satisfaction. To comply with the conventions in the literature, we construct two earnings variables: (1) the natural logarithm of earnings and (2) a dummy variable indicating the earnings rank of the worker, i.e., relative earnings. As we report in Table (2.4)], the average earnings in the reference group is negatively related to the individual job satisfaction score. Moreover, working in a group with a greater fraction workers earning more than the median wage also reduces individual job satisfaction.[22] This is consistent with the findings in the pay-comparisons literature, which suggest that job satisfaction depends on relative income comparisons [see, for example, Clark et al. (2009) and Card et al. (2012)]. Our findings confirm the view that income is evaluated relative to some comparison level based on the reference group and not only in absolute terms. This is in line with the findings reported in the literature [see, e.g., Easterlin (1973)].

[22] So, instead of the Hirschman's tunnel effect, we observe that envy/hatred is more likely to be effective.

For the BHPS analysis, we do not find any statistically significant contextual effects for the Male, Age, Log Earnings, and Relative Earnings variables at the local labor market level, unlike our workplace level analysis. This may be due to the reason that individuals care less about the group-level exogenous characteristics in larger reference groups. However, we do report a different contextual effect at the local labor market level: the promotion opportunities, a variable that does not exist in the WERS data set. Specifically, we find that working in groups with a greater fraction of workers with access to promotion opportunities reduces individual-level job satisfaction [see Table (2.5)]. This can be attributed to competition: that is, if there is a large fraction of workers in one's reference group expecting promotions, then this might increase excess competition in the work environment and, therefore, might reduce job satisfaction. It is interesting, however, detecting this result at the local labor market level. This may be suggesting that working in industries with harsher competition conditions reduces individual-level job satisfaction. We do not have results for this variable at the workplace level; however, if the competition hypothesis is true, we conjecture that the contextual effect of promotion opportunities would be even stronger at the workplace level.

2.4.1.2 The Role of Heterogeneity

The discussion above focuses on the marginal effects of the variables and ignores the magnitudes of the interaction terms. But, the interaction terms in the hierarchical model may themselves yield interesting results. In particular, the interaction terms might suggest whether the endogenous and/or contextual social effects exhibit any heterogeneity, i.e., whether the social interactions estimates depend on the environment in which the associated interactions take place. For the sake of brevity, we perform this task only with the WERS data.

Our "social ecologies" model features two types of interaction terms. First, the $m_g \times Y_g$ interaction indicates whether the endogenous social effect differs across groups with different characteristics. For example, the effect of group-level job satisfaction on individual-level job satisfaction may be different in groups with different gender dominance structures or different education compositions. Second, the $X_{i_g} \times Y_g$ interaction indicates whether the individual-level effects change with respect to group-level characteristics. For example, the relationship between individual-level characteristics and the individual-level job satisfaction scores may be different in groups with different characteristics. Table (2.6) reports the full set of interaction terms in the first group. The results provide interesting insights on the group-level heterogeneity structure. Most importantly, we find that the endogenous social effects are stronger in firms with a larger fraction of older and more educated workers. This suggests that working in a group of individuals with higher human capital levels exposes workers to stronger peer effects in job satisfaction. The second set of interaction terms include so many variables that it would be inefficient to report all of them. Instead, we prefer to mention verbally the most striking estimates. We find some gender effects that worth mentioning. Table (2.4) documents that job satisfaction is higher in groups with a greater fraction of male workers. We find that this effect is much larger for male workers than females. We also show that the negative relative income effect is felt much stronger by males than females. These patterns suggest that both endogenous and contextual social effects are subject to a certain degree of heterogeneity in the worker population.

It remains to check whether the interaction terms are jointly statistically significant or not. This is equivalent to test the relevance of our identifying assumption, which introduces non-linearities to the standard linear-in-means model through incorporating interaction terms as in Equation (2.11). If the interaction terms are jointly insignificant, then the model will favor the linear-in-means model over the hierarchical model; therefore, our model will remain fundamentally unidentified and would be subject to Manski's criticism. We test the joint significance of the interaction terms both for the WERS and BHPS data sets using the F-test. For the WERS, the interaction terms are statistically significant at 1 percent level with a p-value very close to zero. For the BHPS, the interaction terms are still jointly statistically significant, but the level of significance is 5 percent (with a p value around 0.045). This means that the validity of our identifying assumption is roughly confirmed by both of the data sets we use.

2.4.1.3 Linear-in-Means Model

Although the simple test we perform above favors the hierarchical model over the linear-in-means model and although we are aware of the fact that the linear-in-means models are potentially plagued with the reflection problem, it will still be a useful exercise to get estimates from the linear-in-means model to understand the extent to which the reflection problem could contaminate the estimates. The linear specification is given by Equation (2.1), which says that the individual-level job satisfaction is a linear function of individual-level characteristics, group-level characteristics, and group-level job satisfaction. Again, for the sake of brevity, we perform this exercise only with the WERS data set.

Table (2.7) reports the linear-in-means estimates. We compare them to the estimates for the hierarchical model given in Table (2.4). The most striking difference is that the endogenous social effect is 0.423 in the hierarchical model, while it is 0.514 in the linear-in-means model. This difference implies that the linear-in-means model overestimates the endogenous social effect by around 22 percent, which is a non-negligibly large bias. The contextual effects are also different; some differing only in magnitude, some differing in both sign and magnitude. The punchline is that ignoring the reflection problem has a potential to generate large biases in the estimates of social interactions in job satisfaction. The reason is that when individual- and group-level outcomes are simultaneously determined within groups, then the parameter estimates from the linear-in-means model would just be a combination of the underlying structural parameters and the true social effects will be masked. The hierarchical model offers a way to separately identify the social effects.

2.4.1.4 $\pi = 0$ Model

We also estimate the equation (2.11) where $\pi = 0$. If this exercise makes a difference regarding R^2 and the endogenous social effect, then utilizing contextual-dependent coefficients for identification strategy would be beneficial. In this exercise, we simply do not control the interaction term group-level contextual variables and group-level job satisfaction. This equation looks similar to the linear-in-means model except that it includes multiplicative interactions between group-level and individual-level contextual variables. This exercise is performed only using the WERS data set.

$$\omega_{i_g} = \beta_0 + \beta_1 \boldsymbol{X}_{i_g} + \beta_2 \mathbf{Y}_g + J m_g + \pi \boldsymbol{Y_g} m_g + \boldsymbol{Y}_g' \boldsymbol{B} \boldsymbol{X}_{i_g} + u_g + \epsilon_{i_g}, \tag{2.13}$$

Table (2.9) reports the estimation result where $\pi = 0$. Again, we compare these results with the result of the hierarchical model given in Table (2.4). The endogenous social effect is 0.501 for the model that $\pi = 0$ while in the hierarchical model, it is 0.423. We find that such a regression could overestimate the endogenous social effect by around 18% and could alter the estimates for other contextual effects. R^2 of the model is also very small, which signalizes the problem of model fitting.

2.4.2 Robustness Checks

In this subsection, we perform several robustness checks for the results that our hierarchical model produces. We carry out this task in two different ways. First, we calculate the group-level means of the variables using an alternative technique and, second, given the flexibility offered by this alternative formulation, we propose a different way of controlling for group-level heterogeneity. It is perhaps needed to mention at this stage that we are severely constrained by data availability in extending the variety of these robustness exercises. Overall, the two alternative exercises presented below strongly suggest that the social interactions estimates obtained from the baseline hierarchical model [see Table (2.4)] are robust.

2.4.2.1 An Alternative Formulation of Group-Level Variables

Since the group sizes are, on average, small especially for the WERS data set, one might suspect that the social interactions estimates reported in Table (2.4) could depend on the way group-level means are calculated. In our baseline calculations, the group-level means are calculated as the simple arithmetic average of the individual-level variables. In particular, our original formulation assumes that the group-level job satisfaction score that affects individual i's job satisfaction level also includes i's own job satisfaction. This has two potential deficiencies. First, if the groups are small, then this formulation will impose a mechanical positive correlation between the individual- and group-level job satisfaction scores, because a change in the individual-level score will be more likely generate a sizable effect on group-level job satisfaction score. Second, it is also plausible to say that the individual i in group g is exposed to a social effect that is characterized by the mean behavior in the group, where the mean is calculated excluding i. When the groups are large, including or excluding the observation for i in calculating the group-level means should be less of a concern, since the group means will be almost identical in both cases.

Based on these concerns, we reconstruct the group-level averages for each individual, excluding the corresponding individual. We re-estimate the hierarchical model based on these new group-level definitions and the results are presented in Table (2.9) in the form of marginal effects. The endogenous social effect, which is 0.408, is only slightly smaller than the original estimate of 0.423. The qualitative nature of the contextual effects also remain unchanged, with the exception that the coefficients of the human capital variables are slightly smaller and the coefficients of the absolute and relative earnings variables are somewhat larger in Table (2.9) relative to the estimates reported in Table (2.4). These results suggest that our estimates are robust to the changes in the method chosen to calculate the group-level means.

2.4.2.2 Group Fixed Effects

Another potential problem with our baseline estimates is that group-level unobserved heterogeneity is controlled only by assuming a random-effects structure in the error term. By definition, the random-effects model is based on the assumption that the error term is uncorrelated with other covariates. Although, this specification is useful in controlling for the fact that the error term may exhibit within-group correlations, it would be inadequate in capturing the group-level fixed effects, which are unobserved factors correlated with the determinants of individual- and group-level variables. Fortunately, the alternative calculation procedure introduced in Section 2.4.2.1 allows us to incorporate the group-level fixed effects manually. Since the group-level variables are calculated excluding i, these variables offer some variation within the group. When such a variation exists, one can generate group dummies and use these dummies as group fixed effects.[23] We perform this task both for the

[23] To understand this point, suppose that there is a group of 10 workers and each of them has received a score based on a certain test. If you calculate the group-level mean of the test score by including everyone, then the mean test score will be the same for everyone in

WERS and BHPS datasets.

For the WERS analysis, we re-estimate the hierarchical model by incorporating these firm fixed effects in a regression in which the group-level variables are calculated excluding the individual i. Table (2.10) demonstrates the results. Strikingly, we find that the endogenous social effect is estimated to be 0.367, which is somewhat lower than but still reasonably close to our original estimate 0.423. This estimate also supports the qualitative nature of our results, which suggests that the social interactions in job satisfaction are strong at the firm level.[24] The signs and significance levels of other variables are also mostly remained unaltered—except that the coefficient of the relative income variable turned to insignificant after including group-level fixed effects. At the end, we conclude that our original WERS estimates are robust to the inclusion of firm-level fixed effects.

For the BHPS analysis, we again re-performed the estimations for our hierarchical model after incorporating the industry-region fixed effects, where the group-level variables are calculated excluding the individual i. Table (2.11) documents the results. The original estimate for the endogenous social effect was 0.147. We find that this estimate declined slightly and became 0.141 after

the group. If the individual i excludes his own test score in calculating the group-level mean he faces, and if everyone does so, then the group-level means will be different for everyone in the group. When there is individual-level variation in the key group-level variables, then it becomes possible to include group-level fixed effects into the regressions. In the absence of such a variation, the group-level fixed effects would be collinear with the group-level means. In particular, the number of omitted firm dummies would be equal to the number of group-level means included into the regression. Thus, the main coefficients of interest would become uninterpretable.

[24] We would like to note that this approach is also not free of any problems. When the group size is small, the accuracy of controlling for group-level heterogeneity via a dummy variable will be low. So, from a "model selection" viewpoint, it is not clear if the model with firm fixed effects should be preferred to the model that excludes the fixed effects.

including the group-level fixed effects. Again, the qualitative nature of other coefficients remains unaltered. This confirms that our BHPS estimates are robust too.

2.4.2.3 Endogenous Group Sorting

Individuals who are more satisfied job-wise may sort together in the same reference group; however this sorting may not have an influence on the individual-level job satisfaction. Moreover, unobserved characteristics may take place before the sorting of individuals to the reference group, i.e., the value of the firm has suddenly been increased, or a nice manager has arrived very recently. Identifying sorting requires worker-firm match data. In the literature, this issue has been widely investigated under whether or not productive workers are employed more in productive jobs. Abowd et al. (1999) analyze the correlation between firm and worker fixed effects from wage regressions. Based on their method, there are several papers that tried to identify the sorting and to find an insignificant or even negative correlation in fixed effects between workers and firm types [Cahuc et al. (2004)]. These results are extracted from several countries such as France, the U.S., Denmark, and Brazil. These papers suggest that a positive assortative matching between workers and firms does not play a crucial role in the labor market. However, Eeckhout and Kircher (2011) show that the fixed effects approach is not able to identify the strength of sorting. They propose another model to focus on the gain of sorting instead of the sign of sorting, which can be identified from the wage data. The identification comes solely from determining some notion of the size of the set of firms with which a worker matches. In our case, we have some limitations when analyzing the issue. Identifying endogenous sorting requires panel employer-employee match data whereas the WERS dataset is a cross section worker-firm match data. Therefore, our results may suffer from endogenous group sorting.

2.4.2.4 Dependent Variable Check

We re-estimate the hierarchical model under two different dependent variables, i.e., "overall job satisfaction". One way is to construct the variable in a different way in the WERS data set, and the other way is to make a better comparison of the WERS and the BHPS data set by combining the the same type of job satisfaction related questions.

Our first dependent variable check is simply summing the five job satisfaction question and creating a score between 7 and 35[25], which gives more variability across workers. The endogenous social effect of the model is 0.419, which is slightly similar to the original model of 0.423 [See Table (2.12)]. The contextual effects are also parallel with the original estimates.

Our second dependent variable check is to match the following job satisfaction scores in the WERS and the BHPS. The questions that we make use of in the WERS data set are "How satisfied are you with the amount of pay you receive?, the job security?, the work itself?. There are similar questions in the BHPS data set: "How satisfied are you with total pay?, the job security?, the work itself? The endogenous social effect is 0.418 with the new constructed job satisfaction variable in the WERS, and in the BHPS it is 0.14 [See Table

[25]We standardize the score around zero mean and unit variance to achieve consistency.

(2.13) for the WERS and Table(2.14) for the BHPS]. These examples show the robustness of our analyses regarding the dependent variable.

2.4.2.5 WERS—Industry $\times$ Region

Our reference group for the WERS data set is establishment-level; for the BHPS data set, we construct industry $\times$ region cells. Although our main aim is to understand the spillover effects in small and large interaction groups, we would also like to provide an evidence of how the social interaction effect varies with the size of the reference group. For that purpose, we replicate our analysis using the WERS data set with a focus on the same reference group of the BHPS.

In the WERS data sets, we collapse the industries in nine categories to achieve the consistency with the BHPS. The regions are constructed with the same way but in the WERS, Northern Ireland region is not available. At the end, we have nine industries and eleven regions to construct the new reference group. Note that in the WERS data set, the regions are recorded in the establishment-level. When we look at the regional mean of job satisfaction of both data sets in Table (2.3), we observe the similar pattern. London is the region, which has the smallest mean of job satisfaction and Wales is the highest one for both data sets.

Table (2.15) shows the estimation result; the endogenous social effect is 0.26 in industry $\times$ region cells whereas it is recorded at 0.42 with the establishment-level reference group. The endogenous social effect is smaller than the previous reference group; however the spillover effect in job satisfaction is still larger than that in the BHPS data set. One reason could be that the variation in region is smaller than the BHPS, given that the regions are recorded in the workplace-level. However, this exercise still presents a consistency in the results that social interactions in job satisfaction are higher in small groups. When we analyze the contextual effects, the results in general are parallel with the previous reference group—establishment-level— however the magnitude is small.

2.4.2.6 Alternative Formulation of Reference Group

Another exercise that we run based on the reference group excludes the worker's own establishment in region $\times$ industry cells. The main reason of this exercise is to decompose the effect coming from worker's own establishment and the effect coming from region $\times$ industry level. Since workers can interact outside of their workplace in the BHPS data set, this may lead to more comprehensive estimates from the WERS data set. Thereby, we first analyze the reference group regarding region $\times$ industry, and then to decompose the effect, we reform the reference group by excluding the worker's own establishment.

We estimate the hierarchical model for the WERS data set, where the group-level variables are calculated by region $\times$ industry cells excluding the individual's own establishment. The endogenous social effect is 0.14 [see Table (2.16)], which is lower than the previous estimate and is focused on region $\times$ industry cells. The previous estimate is 0.26: this suggests that 0.12 is coming from the establishment-level. This also supports the argument that social interactions in job satisfaction are strong at the firm level.

2.4.2.7 Establishment Size

There is a possibility that small establishments drive the result of higher endogenous social effect in the workplace. To understand this, we reestimate the hierarchical model across firm size: small firms, which are the number of workers less than 50; medium firms are between 50 and 200 workers; and large firms, which are higher than 200 workers. The endogenous social effect for small firms, which is 0.432, is only slightly bigger than the original estimate of 0.423. The endogenous social effect for medium and large establishments are 0.415 and 0.410 respectively. The contextual effects yield the same result. These results say that our estimates are robust across firm size[26].

2.4.2.8 Job Satisfaction Components

To have a better understanding of the relative effect of the job satisfaction component, we re-estimate the hierarchical model by Shapley decomposition. The decomposition is estimated to find out what the marginal contributions of different variables are. We apply the Shapley decomposition, by keeping the individual job satisfaction as a composite index, and utilize the group average values and interaction terms of job satisfaction components such as the sense of achievement you get from the work, the scope for using your own initiative, the amount of influence you have over the job, the training you receive, the amount of pay you receive, the job security, and the work itself along with other regressors. Shapley decomposition allows to consider all the possible sequences to eliminate the explanatory variables.

[26]The results are available upon request.

With the help of this example, we can observe which job satisfaction component is more prominent in social interactions. We conduct this exercise only with the WERS dataset. The greatest marginal impacts are "the amount of pay you receive" and "the training you receive"[27]. Among these job satisfaction components, the impact of the satisfaction with the scope for using your own initiative is the weakest. These results suggest that social interactions in job satisfaction are mostly driven by the satisfaction of payment. This indeed asks the questions of whether this effect is coming from the "Hirschman's Tunnel Effect" or income-comparison. This exercise has very interesting results and opens doors for future research.

2.4.3 Policy Implications

It will perhaps be useful to put the policy implications of our results in a nutshell. These results suggest that (1) there are large gains to policy interventions to increase individual-level job satisfaction as there are significant positive feedback effects from group-level job satisfaction toward individual-level job satisfaction in the form of spillover externalities; (2) failing to account for the spillover externalities in job satisfaction may lead us to mis-assess the effectiveness of job satisfaction policies; thus, the policy maker should internalize these externalities; and (3) job satisfaction spillovers are much stronger at the workplace level than local labor market level: therefore, designing/enforcing job satisfaction policies at the workplace level will likely be more effective than implementing such policies at the local labor market level.

[27]The results are available upon request.

Macroeconomic factors such as GDP per capita, inflation, and unemployment have an influence on the subjective well-being of the individual; the evidence comes from a quarter of a million randomly sampled Europeans and Americans from the 1970s to the 1990s [Di Tella et al. (2003)]. Since the subjective well-being of the individual can be altered with the macroeconomic factor, the same can also apply within the firm. In general, the situation of the firm —such as profitability— has an impact on the workers' job satisfaction [Yee et al. (2008)]. Since the social interactions in job satisfaction in the workplace are high, the impact of the situation of the firm on the worker's job satisfaction may be even higher than that. Moreover, there is vast literature focusing on the implementation of teamwork to increase productivity in the workplace [Hoegl and Gemuenden (2001)]. Since there is a significant and positive relationship between job satisfaction and productivity [Boeckerman and Ilmakunnas (2012)], one may increase the efficiency in team work not only to increase the job satisfaction of the individual but also through to increase social interaction among workers. As we observed from the break-down of the job satisfaction components, social interactions in job satisfaction are mostly induced by satisfaction from payment. This result has a possible labor policy market implication after analyzing whether this satisfaction is coming from income-comparison or the "Hirschman's Tunnel Effect".

Although we find that the job satisfaction spillovers are particularly strong at the workplace level, our results also suggest that it would be misleading to exclude the possibility of spillovers in broader reference groups. In other words, regional aspects of working life and conditions in the local labor markets may also induce interactions among people that can exhibit non-negligible social effects.

2.5 Concluding Remarks

There is a large literature arguing that peer effects exist in various labor market outcomes including productivity, wages, absenteeism, and learning (or knowledge spillovers). We contribute to this literature in three ways. First, this is the first paper in the literature testing the existence of job satisfaction spillovers. We show that there exist significant positive spillovers in job satisfaction. Second, we perform our analysis at two different aggregation levels using two different data sets from the United Kingdom. We find that the job satisfaction spillovers are almost three times stronger at the workplace level than local labor market level (defined in terms of industry $\times$ region cells). This implies that although job satisfaction spillovers are strong among narrowly defined worker groups, it would be misleading to exclude the possibility of spillovers in broader reference groups. In other words, regional aspects of working life and conditions in the local labor markets may also induce interactions among people that can exhibit non-negligible social effects. Finally, we make a methodological contribution to the empirical literature by resolving the identification problem using an intuitive insight from the hierarchical models of social processes. Specifically, we hypothesize that our parameters of interest are determined within the social environments they originate from. Under reasonable specifications, this logic implies introducing certain cross-product terms into the standard estimating equations.

We conclude that there are sizable social interactions in job satisfaction that should not be ignored in assessing policy effectiveness. The policy makers should internalize these spillover externalities. Our estimates also provide guidance on the question "at which level job satisfaction spillovers should be internalized." We argue that firms should design and implement their own job satisfaction policies rather than relying on more general policies or institutional regulations that could only be enforced at the local labor market level (or industry level).

Table 2.1: Summary Statistics—WERS

Variable	Mean	Std.Dev
Job satisfaction	4.20	2.11
Std. job satisfaction	1.07e-08	1
Individual-level Characteristics		
Male	0.469	0.499
Married	0.682	0.466
Age	41.646	12.095
Higher degree	0.009	0.092
First degree	0.033	0.18
'A'-level	0.273	0.446
'O'-level	0.238	0.426
Vocational qual.	0.446	0.497
Log earnings	5.692	0.74
Relative earnings	0.518	0.5
Working hours	0.656	0.475
Job-level Characteristics		
Private sector	0.365	0.481
Public sector	0.551	0.497
Union membership	0.462	0.499
Small-size establishment	0.324	0.468
Medium-size establishment	0.318	0.466
Large-size establishment	0.358	0.48
# of observations	20,035	
# of workplaces/establishments	1,673	

Notes: Workplace Employment Relations Survey 2004 data set is used to construct this table.

Table 2.2: Summary Statistics—BHPS

Variable	Mean	Std.Dev
Job satisfaction	5.40	1.26
Std. job satisfaction	-3.13e-09	1
Individual-level Characteristics		
Male	0.474	0.499
Married	0.573	0.495
Age	40.351	12.049
Higher degree	0.044	0.205
First degree	0.158	0.365
‘A’-level	0.126	0.332
‘O’-level	0.176	0.381
Other higher qual.	0.308	0.462
Vocational qual.	0.094	0.292
No qual.	0.094	0.291
Log earnings	6.907	0.707
Relative earnings	0.531	0.499
Job-level Characteristics		
Union membership	0.331	0.412
Promotion opportunities	0.499	0.5
Full-time job	0.79	0.407
Private sector	0.63	0.48
Public sector	0.22	0.42
Prefer to work fewer hours	0.328	0.47
Prefer to work more hours	0.054	0.226
Prefer to contain same hours	0.618	0.486
Small-size establishment	0.486	0.5
Medium-size establishment	0.216	0.411
Large-size establishment	0.299	0.458
Industries		
Energy-water supplies	0.036	0.186
Extraction-manufacture	0.074	0.262
Metal goods-engineering	0.046	0.21
Other manufacturing	0.058	0.235
Construction	0.178	0.383
Distribution, hotels, catering	0.096	0.295
Transport-communication	0.196	0.397
Banking-finance	0.254	0.435
Other services	0.061	0.239
Regions		
London	0.05	0.219
South East	0.114	0.318
South West	0.054	0.226
East Anglia	0.024	0.154
East Midlands	0.052	0.222
West Midlands	0.046	0.209
Northwest	0.066	0.249
Yorkshire-Humberside	0.054	0.226
North East	0.037	0.189
Wales	0.148	0.355
Scotland	0.188	0.391
Northern Ireland	0.166	0.372
# of observations	6,428	

Notes: British Household Panel Survey 2004 cross-section is used to construct this table.

Table 2.3: Summary Statistics—Job Satisfaction score over region

Job Satisfaction	**WERS**		**BHPS**	
	Mean	**Std.Dev**	**Mean**	**Std.Dev**
London	4.33	0.14	5.28	0.08
South East	4.57	0.08	5.38	0.05
South West	4.55	0.09	5.38	0.06
East Anglia	4.30	0.09	5.31	0.13
East Midlands	4.64	0.01	5.51	0.06
West Midlands	4.35	0.21	5.44	0.77
Northwest	4.55	0.07	5.32	0.07
Yorkshire-Humberside	4.40	0.09	5.38	0.71
North East	4.33	0.14	5.31	0.92
Wales	4.65	0.12	5.44	0.47
Scotland	4.34	0.08	5.34	0.41
Northern Ireland	-	-	5.57	0.04

Std. Job Satisfaction	**WERS**		**BHPS**	
	Mean	**Std.Dev**	**Mean**	**Std.Dev**
London	0.05	0.04	-0.01	0.06
South East	0.17	0.04	-0.02	0.03
South West	0.16	0.04	-0.02	0.05
East Anglia	0.05	0.04	-0.08	0.10
East Midlands	0.21	0.05	0.09	0.05
West Midlands	0.7	0.04	0.03	0.06
Northwest	0.16	0.03	-0.07	0.05
Yorkshire-Humberside	0.09	0.04	-0.02	0.05
North East	0.06	0.07	-0.07	0.07
Wales	0.21	0.06	0.03	0.03
Scotland	0.07	0.04	-0.05	0.03
Northern Ireland	-	-	0.13	0.03

Table 2.4: Estimation results—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.423***	(0.007)
Individual characteristics				
Male	-0.160***	(0.017)	0.157***	(0.038)
Married	0.063***	(0.016)	-0.075	(0.051)
Age	-0.031***	(0.005)	0.026**	(0.013)
Age-squared/100	0.039***	(0.005)	-0.034**	(0.015)
First degree	0.147	(0.161)	0.094	(0.341)
'A'-level	0.265*	(0.150)	-0.073	(0.307)
'O'-level	0.298**	(0.500)	-0.099	(0.305)
Vocational qual.	0.430***	(0.149)	-0.211	(0.302)
Log earnings	0.232***	(0.022)	-0.203***	(0.032)
Relative earnings	0.083***	(0.021)	-0.093*	(0.057)
Working hours	-0.076***	(0.020)	0.047	(0.044)
Establishment/job characteristics				
Private sector	–	–	-0.003	(0.028)
Public sector	–	–	-0.004	(0.027)
Not union member	–	–	-0.015	(0.030)
Medium-size establishment	–	–	-0.003	(0.017)
Large establishment	–	–	-0.006	(0.017)
# of observations		20,035		
R-squared		0.1172		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects." Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.5: Estimation results—BHPS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.147***	(0.028)
Individual characteristics				
Male	-0.172***	(0.030)	0.328	(0.244)
Married	0.161***	(0.027)	0.368	(0.303)
Age	-0.028***	(0.007)	0.094	(0.070)
Age-squared/100	0.039***	(0.009)	-0.144*	(0.084)
Higher degree	0.262**	(0.102)	-1.033	(0.771)
First degree	0.194**	(0.099)	-1.048	(0.742)
'A'-level	0.142	(0.091)	-0.701	(0.602)
'O'-level	0.235**	(0.094)	-0.889	(0.671)
Other higher qual.	0.137	(0.096)	-0.888	(0.788)
Vocational qual.	0.052	(0.094)	-1.152	(0.790)
Log earnings	0.119***	(0.035)	-0.198	(0.193)
Relative earnings	0.111***	(0.038)	-0.233	(0.423)
Job characteristics				
Private sector	-0.025	(0.083)	0.196	(0.272)
Public sector	-0.127	(0.096)	0.082	(0.287)
Union membership	-0.060*	(0.033)	0.178	(0.200)
Promotion opportunities	0.282***	(0.026)	-0.430*	(0.236)
Full-time job	-0.183***	(0.044)	-0.244	(0.445)
Prefer to work fewer hours	-0.393***	(0.027)	0.276	(0.279)
Medium-size establishment	-0.181***	(0.033)	0.310	(0.337)
Large establishment	-0.162***	(0.033)	-0.052	(0.273)
# of observations		6,428		
R-squared		0.1098		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects."

Table 2.6: Group-level interaction terms—WERS

Dependent variable: job satisfaction score (standardized)		
INTERACTION TERMS ($m_g \times Y_g$) – Hierarchical model		
Covariate	**Coefficient**	**(St. Error)**
Individual characteristics		
$m_g \times Y_g$ (Male)	0.012	(0.085)
$m_g \times Y_g$ (Married)	0.011	(0.103)
$m_g \times Y_g$ (Age)	0.044*	(0.024)
$m_g \times Y_g$ (Age-squared/100)	-0.049*	(0.029)
$m_g \times Y_g$ (First degree)	0.928*	(0.467)
$m_g \times Y_g$ ('A'-level)	0.847*	(0.416)
$m_g \times Y_g$ ('O'-level)	0.864	(0.567)
$m_g \times Y_g$ (Vocational qual.)	0.710*	(0.386)
$m_g \times Y_g$ (Log earnings)	0.039	(0.054)
$m_g \times Y_g$ (Relative earnings)	-0.119	(0.096)
$m_g \times Y_g$ (Working hours)	-0.151*	(0.083)
Establishment/job characteristics		
$m_g \times Y_g$ (Private sector)	0.116	(0.081)
$m_g \times Y_g$ (Public sector)	0.042	(0.078)
$m_g \times Y_g$ (Not union member)	0.127*	(0.065)
$m_g \times Y_g$ (Medium-size establishment)	-0.122**	(0.049)
$m_g \times Y_g$ (Large establishment)	-0.079	(0.053)
# of observations	20,035	

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for.

Table 2.7: Linear-in-means model—WERS

Dependent variable: job satisfaction score (standardized)				
Linear-in-means model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.514***	(0.020)
Individual characteristics				
Male	-0.141***	(0.017)	-0.031	(0.030)
Married	0.083***	(0.016)	0.076**	(0.037)
Age	-0.029***	(0.004)	0.008	(0.009)
Age-squared/100	0.039***	(0.005)	-0.009	(0.011)
First degree	-0.062	(0.080)	-0.231	(0.212)
'A'-level	0.031	(0.072)	-0.277	(0.178)
'O'-level	0.039	(0.074)	-0.445**	(0.176)
Vocational qual.	0.170**	(0.074)	-0.439**	(0.173)
Log earnings	0.136***	(0.020)	-0.099***	(0.026)
Relative earnings	0.137***	(0.021)	-0.016	(0.046)
Working hours	-0.066***	(0.020)	0.031	(0.034)
Establishment/job characteristics				
Private sector	–	–	-0.018	(0.020)
Public sector	–	–	-0.044**	(0.018)
Not union member	–	–	-0.102***	(0.021)
Medium-size establishment	–	–	-0.032**	(0.013)
Large establishment	–	–	-0.070***	(0.014)
# of observations		20,035		
R-squared		0.083		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.8: π=0—WERS

Dependent variable: job satisfaction score (standardized)				
Linear-in-means model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.501***	(0.016)
Individual characteristics				
Male	-0.150***	(0.017)	0.145***	(0.040)
Married	0.070***	(0.016)	0.068**	(0.040)
Age	-0.031***	(0.004)	0.008	(0.009)
Age-squared/100	0.039***	(0.005)	-0.009	(0.011)
First degree	0.120	(0.140)	0.080	(0.207)
'A'-level	0.200	(0.190)	-0.103	(0.178)
'O'-level	0.210	(0.119)	-0.225**	(0.176)
Vocational qual.	0.296**	(0.500)	-0.239**	(0.173)
Log earnings	0.200***	(0.020)	-0.105***	(0.026)
Relative earnings	0.103***	(0.021)	-0.090***	(0.025)
Working hours	-0.070***	(0.020)	0.060**	(0.034)
Establishment/job characteristics				
Private sector	–	–	-0.010	(0.025)
Public sector	–	–	-0.033*	(0.025)
Not union member	–	–	-0.083**	(0.016)
Medium-size establishment	–	–	-0.026**	(0.013)
Large establishment	–	–	-0.034**	(0.014)
# of observations		20,035		
R-squared		0.090		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.9: Group means exclude i—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model with group means excluding i.				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.408***	(0.008)
Individual characteristics				
Male	-0.143***	(0.021)	0.181***	(0.041)
Married	0.086***	(0.020)	-0.069	(0.053)
Age	-0.038***	(0.006)	0.028**	(0.014)
Age-squared/100	0.044***	(0.006)	-0.036**	(0.016)
First degree	0.122	(0.172)	0.083	(0.366)
'A'-level	0.192	(0.177)	-0.084	(0.357)
'O'-level	0.243**	(0.487)	-0.109	(0.352)
Vocational qual.	0.380**	(0.195)	-0.254	(0.342)
Log earnings	0.245***	(0.021)	-0.211***	(0.033)
Relative earnings	0.097***	(0.022)	-0.096*	(0.058)
Working hours	-0.066***	(0.018)	0.039	(0.046)
Establishment/job characteristics				
Private sector	–	–	-0.001	(0.032)
Public sector	–	–	-0.005	(0.029)
Not union member	–	–	-0.028	(0.034)
Medium-size establishment	–	–	0.004	(0.022)
Large establishment	–	–	-0.002	(0.021)
# of observations		20,035		
R-squared		0.1165		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects." Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.10: Group means exclude i, firm fixed effects included—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model with group means excluding i.				
Firm fixed effects are included.				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.367***	(0.026)
Individual characteristics				
Male	-0.096***	(0.026)	0.149***	(0.051)
Married	0.081***	(0.025)	-0.043	(0.062)
Age	-0.032***	(0.010)	0.023*	(0.015)
Age-squared/100	0.035***	(0.011)	-0.027*	(0.017)
First degree	0.099	(0.192)	0.052	(0.392)
‘A’-level	0.161	(0.197)	-0.061	(0.422)
‘O’-level	0.203	(0.381)	-0.089	(0.386)
Vocational qual.	0.320*	(0.191)	-0.199	(0.401)
Log earnings	0.211***	(0.027)	-0.188***	(0.057)
Relative earnings	0.091***	(0.024)	-0.085	(0.072)
Working hours	-0.056**	(0.029)	0.029	(0.051)
Establishment/job characteristics				
Private sector	–	–	-0.001	(0.037)
Public sector	–	–	0.012	(0.046)
Not union member	–	–	-0.041	(0.066)
Medium-size establishment	–	–	-0.016	(0.052)
Large establishment	–	–	0.007	(0.053)
Firm fixed effects	**Included**			
# of observations	20,035			
R-squared	0.1163			

Notes:*, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., firm) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the “marginal effects.” Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics. Firm-level fixed effects are included.

Table 2.11: Group means exclude i, industry-region cell fixed effects included—BHPS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
Industry-region cell fixed effects are included.				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.141***	(0.031)
Individual characteristics				
Male	-0.151***	(0.036)	0.216	(0.301)
Married	0.146***	(0.031)	0.221	(0.441)
Age	-0.027***	(0.008)	0.054	(0.091)
Age-squared/100	0.037***	(0.010)	-0.066	(0.097)
Higher degree	0.251**	(0.107)	-1.201	(0.894)
First degree	0.183*	(0.111)	-1.103	(0.801)
'A'-level	0.110	(0.103)	-0.755	(0.792)
'O'-level	0.216*	(0.124)	-0.904	(0.687)
Other higher qual.	0.109	(0.122)	-1.001	(0.904)
Vocational qual.	0.021	(0.127	-1.202	(0.905)
Log earnings	0.111***	(0.037)	-0.123	(0.240)
Relative earnings	0.104***	(0.040)	-0.175	(0.501)
Job characteristics				
Private sector	-0.029	(0.084)	0.101	(0.303)
Public sector	-0.110	(0.106)	0.044	(0.401)
Union membership	-0.029	(0.041)	0.099	(0.386)
Promotion opportunities	0.211***	(0.052)	-0.211	(0.388)
Full-time job	-0.141***	(0.054)	-0.178	(0.489)
Prefer to work fewer hours	-0.288***	(0.051)	0.200	(0.476)
Medium-size establishment	-0.149***	(0.037)	0.234	(0.356)
Large establishment	-0.141***	(0.036)	-0.012	(0.298)
Industry-region fixed effects		**Included**		
# of observations		6,428		
R-squared		0.1065		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects." Group-level fixed effects are included.

Table 2.12: Job Satisfaction Score (7-35)—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.419***	(0.007)
Individual characteristics				
Male	-0.158***	(0.016)	0.154***	(0.038)
Married	0.065***	(0.016)	-0.072	(0.050)
Age	-0.029***	(0.005)	0.024**	(0.012)
Age-squared/100	0.039***	(0.005)	-0.034**	(0.015)
First degree	0.152	(0.160)	0.090	(0.343)
‘A’-level	0.262*	(0.152)	-0.077	(0.305)
‘O’-level	0.300**	(0.502)	-0.098	(0.303)
Vocational qual.	0.433***	(0.150)	-0.212	(0.301)
Log earnings	0.229***	(0.023)	-0.201***	(0.032)
Relative earnings	0.080***	(0.022)	-0.090*	(0.057)
Working hours	-0.073***	(0.020)	0.045	(0.043)
Establishment/job characteristics				
Private sector	–	–	-0.003	(0.026)
Public sector	–	–	-0.005	(0.025)
Not union member	–	–	-0.016	(0.031)
Medium-size establishment	–	–	-0.003	(0.017)
Large establishment	–	–	-0.007	(0.017)
# of observations		20,035		
R-squared		0.1168		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the “marginal effects.” Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.13: Aggregation of Job Satisfaction Components—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.418***	(0.007)
Individual characteristics				
Male	-0.164***	(0.017)	0.155***	(0.041)
Married	0.068***	(0.017)	-0.073	(0.052)
Age	-0.030***	(0.006)	0.027**	(0.013)
Age-squared/100	0.039***	(0.005)	-0.034**	(0.015)
First degree	0.170**	(0.073)	0.111*	(0.051)
‘A’-level	0.190**	(0.099)	-0.040**	(0.037)
‘O’-level	0.213**	(0.0.94)	-0.050	(0.033)
Vocational qual.	0.235**	(0.095)	-0.034	(0.032)
Log earnings	0.262***	(0.103)	-0.223***	(0.032)
Relative earnings	0.099***	(0.035)	-0.089*	(0.057)
Working hours	-0.070***	(0.023)	0.052	(0.043)
Establishment/job characteristics				
Private sector	–	–	-0.002	(0.024)
Public sector	–	–	-0.005	(0.026)
Not union member	–	–	-0.017	(0.030)
Medium-size establishment	–	–	-0.003	(0.016)
Large establishment	–	–	-0.009	(0.020)
# of observations		20,035		
R-squared		0.1160		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the “marginal effects.” Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also the firm- or group-level characteristics) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.14: Aggregation of Job Satisfaction Components—BHPS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.140***	(0.030)
Individual characteristics				
Male	-0.160***	(0.035)	0.220	(0.303)
Married	0.150***	(0.030)	0.205	(0.442)
Age	-0.028***	(0.007)	0.054	(0.093)
Age-squared/100	0.035***	(0.009)	-0.063	(0.095)
Higher degree	0.253**	(0.106)	-1.207	(0.878)
First degree	0.180*	(0.110)	-1.110	(0.860)
'A'-level	0.109	(0.100)	-0.750	(0.787)
'O'-level	0.212*	(0.120)	-0.809	(0.680)
Other higher qual.	0.103	(0.120)	-0.979	(0.923)
Vocational qual.	0.020	(0.128)	-1.223	(0.925)
Log earnings	0.113***	(0.035)	-0.125	(0.220)
Relative earnings	0.101***	(0.039)	-0.170	(0.488)
Job characteristics				
Private sector	-0.032	(0.088)	0.102	(0.301)
Public sector	-0.115	(0.109)	0.043	(0.405)
Union membership	-0.027	(0.043)	0.069	(0.366)
Promotion opportunities	0.210***	(0.050)	-0.199	(0.364)
Full-time job	-0.139***	(0.055)	-0.164	(0.449)
Prefer to work fewer hours	-0.300***	(0.049)	0.183	(0.446)
Medium-size establishment	-0.153***	(0.040)	0.223	(0.346)
Large establishment	-0.140***	(0.039)	-0.010	(0.248)
Industry-region fixed effects	**Included**			
# of observations	6,428			
R-squared	0.1088			

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects."

Table 2.15: Industry × Region—WERS

Dependent variable: job satisfaction score (standardized)				
MARGINAL EFFECTS – Hierarchical model				
	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Endogenous social effect				
Mean job satisfaction (std)	–	–	0.26***	(0.029)
Individual characteristics				
Male	-0.121***	(0.047)	0.133	(0.531)
Married	0.265***	(0.029)	0.400	(0.361)
Age	-0.029***	(0.009)	0.051	(0.091)
Age-squared/100	0.043***	(0.010)	-0.034	(0.131)
First degree	0.141*	(0.079)	-1.139	(0.826)
‘A’-level	0.102	(0.059)	-0.842	(0.792)
‘O’-level	0.97	(0.054)	-0.853	(0.763)
Vocational qual.	0.052	(0.033)	-1.378	(0.935)
Log earnings	0.130***	(0.046)	-0.324***	(0.141)
Relative earnings	0.109***	(0.049)	-0.199*	(0.105)
Job characteristics				
Private sector	–	–	-0.002	(0.024)
Public sector	–	–	-0.005	(0.026)
Not union member	–	–	-0.017	(0.030)
Medium-size establishment	–	–	-0.003	(0.016)
Large establishment	–	–	-0.009	(0.020)
# of observations		20,035		
R-squared		0.1155		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the “marginal effects.” Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also group-level characteristics because industry × region cells are recorded in the firm-level.) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

Table 2.16: Alternative Formulation of Reference Group—WERS

Dependent variable: job satisfaction score (standardized)

MARGINAL EFFECTS – Hierarchical model

	Individual level		**Group level**	
Covariate	**Coefficient**	**(St. Error)**	**Coefficient**	**(St. Error)**
Mean job satisfaction (std)	–	–	0.14***	(0.027)
Individual characteristics				
Male	-0.110***	(0.048)	0.104	(0.423)
Married	0.132**	(0.059)	0.386	(0.343)
Age	-0.024***	(0.005)	0.049	(0.091)
Age-squared/100	0.040***	(0.011)	-0.029	(0.108)
First degree	0.120**	(0.054)	-1.125	(0.816)
'A'-level	0.76	(0.053)	-0.830	(0.783)
'O'-level	0.169	(0.142)	-0.860	(0.745)
Vocational qual.	0.032	(0.021)	-1.370	(0.924)
Log earnings	0.121***	(0.028)	-0.303***	(0.138)
Relative earnings	0.110***	(0.015)	-0.190*	(0.97)
Job characteristics				
Private sector	–	–	-0.001	(0.022)
Public sector	–	–	-0.005	(0.028)
Not union member	–	–	-0.021	(0.033)
Medium-size establishment	–	–	-0.001	(0.014)
Large establishment	–	–	-0.006	(0.019)
# of observations		20,035		
R-squared		0.1153		

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the group (e.g., industry-region) level, are reported in parentheses. Group-level unobserved effects are controlled for. The first two columns document the estimates for individual-level coefficients, while the last two columns document those for group-level coefficients (e.g., the social interactions estimates). The coefficients should be interpreted as the "marginal effects." Since the individual- and group-level variables are the same for WERS (i.e., the characteristics of the firm that the worker is employed is also group-level characteristics because industry × region cells are recorded in the firm-level.) we drop the individual-level coefficients and report only group-level estimates for the establishment/job characteristics.

CHAPTER 3

DAY-OF-THE-WEEK EFFECTS IN SUBJECTIVE WELL-BEING: DOES SELECTIVITY MATTER?

3.1 Introduction

There is a vast literature documenting significant day-of-the-week effects in subjective well-being. Empirical studies find that individuals tend to report lower levels of happiness on Sundays and/or Mondays, whereas they tend to report higher job satisfaction levels on Fridays and/or Saturdays than the other days of the week. Recent breakthrough studies confirming the empirical relevance of the day-of-the-week effects in this literature include Taylor (2006), Akay and Martinsson (2009), and Helliwell and Wang (2013)[1]. These are

[1]Specifically, Taylor (2006) uses the BHPS data and documents that those who are interviewed on Fridays report higher levels of job satisfaction and lower levels of mental stress than those interviewed in the middle of the week. Akay and Martinsson (2009) test the same hypothesis using the GSOEP data and the result yields a 'blue" Sunday. Helliwell and Wang (2013) utilize the Gallup/Healthways U.S. daily poll to examine the differences in the dynamics of two key measures of subjective well-being: emotions and life evaluation. They find no day-of-the-week effect for life evaluations, but significantly more happiness, enjoyment, and laughter; while significantly less worry, sadness, and anger on weekend than on weekdays. Earlier studies on this topic include Rossi and Rossi (1977), Stone, Hedges, Neale, and Satin (1985), Kennedy-Moore, Greenberg, Newman, and Stone (1992) and Egloff, Tausch, Kohlmann, and Krohne (1995). See Csikszentmihalyi and Hunter (2003)

the widely-agreed day-of-the-week patterns extracted from the main micro-level datasets including large-scale ones such as the British Household Panel Survey (BHPS), the German Socio-Economic Panel Survey (GSOEP), and Gallup/Healthways polls as well as several small-scale surveys. The observed patterns are often attributed to the "circaseptum rhytms" (i.e., seven day cycles) hypothesis studied in the behavioral psychology literature [Larsen and Kasimatis (1990, 1991) and Croft and Walker (2001)]. Overall, this literature suggests that well-being is subject to mood fluctuations and has a highly state-dependent nature.

These findings have important implications for economic modeling. The abstract concept of "utility" is at the heart of modern economics, but the main problem with this concept is that there is no direct measure of utility. Instead, several proxies are used to measure utility. In particular, self-reported well-being is a widely-agreed proxy on various aspects of individual utility. For example, Frey and Stutzer (2002) and Clark, Frijters, and Shields (2008) argue that self-reported happiness scores can be used as a general measure to proxy individual-level utility and they provide detailed reviews of the related literature. Similarly, Clark and Oswald (1996) argue that the self-reported job satisfaction score is a direct measure of individual-level utility derived from the current job. The results reported in the day-of-the-week effects literature imply that utility—as it is proxied by the subjective well-being scores—depends on the events and circumstances that affect individuals even for only a very short period of time. In other words, this literature suggests that utility is not

and Pettengill (2003) for literature surveys.

necessarily stable and it is subject to mood fluctuations[2]. The main principle behind this argument is that individuals assess their well-being at any given moment over time [Kahneman, Diener, and Schwarz (1999)]. However, this is in stark contrast with the neoclassical tradition—in particular, the Beckerian tradition—assuming stable preferences that do not often change over time and across states [Becker (1976)]. Although the stable preferences assumption is no longer a rigid requirement of neoclassical analysis[3], there is still considerable emphasis on preferences that do not quickly change over time or across states—otherwise, every economic phenomenon could be explained by quickly changing preferences, which would easily be labelled as a tautological statement.

In this paper, we ask if these day-of-the-week estimates for job satisfaction and happiness measures suffer from selection bias. We focus on two well-being categories: happiness and job satisfaction. Sundays and/or Mondays are often regarded as "blue", so individuals are, on average, unhappy on these days. Fridays and/or Saturdays are the days in which self-reported job satisfaction is, on average, the highest. This is a sensible question because it may well be the case that individuals who are interviewed on Fridays and Saturdays are mostly the ones who enjoy working hard during the week and more relaxed days like Fridays or Saturdays are the only available time for them to respond the survey. Similarly, it may be the case that individuals who are interviewed on Sundays represent an oversample of those doing housework and, thus, tend

[2]That individual-level well-being significantly varies across the days of the week is an extreme version of short-term state dependency.

[3]See Becker (1976)

to report lower happiness levels. Alternatively, individuals who are not working hard throughout the week can prefer to take the survey on Sundays instead of resting. On Mondays, responding the survey could be a good reason for procrastination due to the overload of beginning of new week. These types of individuals can be unsatisfied with their jobs or their lives in general. If there is selectivity, then this would weaken the argument that individual-level "mood" regularly fluctuates over the days of the week. Instead, the existence of selectivity would suggest that the changes in self-reported well-being scores over the week likely come from the changes in the composition of interviewees over the week based on their unobserved characteristics.

We employ a standard Heckman (1979) selection-correction procedure to test the existence of selection bias. In other words, we formally examine if subjective well-being is correlated with unobserved characteristics that lead the individuals to take the interview on some specific days of the week. We find significant positive selection both for job satisfaction and happiness measures. Specifically, we find that the ones interviewed on Fridays and Saturdays tend to report higher job satisfaction than a random sample drawn from the population of employed workers with a comparable set of observed characteristics would report. For happiness, we find that those interviewed on Sundays and Mondays tend to report lower happiness levels than a random sample drawn from the population of employed workers with a comparable set of observed characteristics would report.

We move one step further and calculate various treatment effects using the techniques summarized by Heckman and Vytlacil (2007a,b), which enables us to attribute causal meanings to our estimates. We focus on average treatment effect (ATE), treatment on the treated (TT), and treatment on the untreated (TUT). In our context, "treatment" refers to taking the interview on a Friday or Saturday for job satisfaction analysis and Sunday or Monday for happiness analysis. We find that the average TT is significantly larger than the ATE, indicating that there is non-random sorting on unobservables across the days of the week. The difference between TT and ATE is a sorting gain [Heckman and Vytlacil (2005)]; that is, TT is the average gain for those who sort into treatment compared to what the average person would gain. For job satisfaction, our results suggest that, on average, individuals who are interviewed on a Friday or Saturday have unobserved characteristics that make them more likely to choose Friday or Saturday as the day of interview. Similarly, for happiness, individuals who are interviewed on a Sunday or Monday have unobserved characteristics that make them more likely to choose Sunday or Saturday as the day of interview.

We conclude that the day of the week effects reported in the literature are likely to be biased and, therefore, should be treated cautiously. Our interpretation of this result is that there is a considerable individual-level unobserved heterogeneity determining well-being scores, and the compositional changes in interviewees in terms of these heterogeneous factors drive most of the observed differences is self-reported well-being across the days of the week. Our findings suggest that the magnitude of the selection bias originated from these compositional shifts is so large that there is only little room for the "mood fluctuations" argument.

The selection-corrected estimates of the determinants of self-reported job satisfaction and happiness scores allow us to understand the channels through which the selection process operates. For job satisfaction, gender, marital status, and job permanency status are the main channels. In the raw regressions, females, married workers, and permanent workers are more satisfied with their jobs than males, non-married workers, and temporary workers. In selection-corrected regressions, males, non-married workers, and temporary workers interviewed on a Friday are Saturday are systematically more satisfied jobwise than the ones interviewed on the remaining days. For happiness, education, job contractual term, being a public versus private sector worker, firm size, and relative income are the main channels through which the selection process operates. The selection-corrected estimates suggest that workers with graduate education, seasonal workers, public sector workers, workers in small firms, and workers earning relatively less than the others in their reference groups who are interviewed on a Sunday or Monday are systematically less happy than the ones interviewed on the remaining days. After correcting for selectivity, we observe that the well-documented results in the empirical literature—such as, married are happier than non-married; there is a U-shaped relationship between age and well-being; women report higher well-being than men, etc.—suffer from selection bias.

The plan of the paper is as follows. Section 3.2 summarizes our data. Section 3.3 describes our econometric model and formulates the selection-correction procedure as well as the treatment effect parameters. Section 3.4 discusses the results in depth. Section 3.5 concludes.

3.2 Data

We use the latest (2008) release of the British Household Panel Survey (BHPS) in our analysis. The BHPS provides information on individual, household, and job/employer related characteristics from 1991 to 2008 in the Great Britain, Scotland, Wales, and Northern Ireland. It yearly follows a nationally representative sample of households, interviews every adult member of sampled households, and assigns a unique identification number for each interviewer. The date of interview is recorded as day-month-year; the day-of-the-week on which an interview occurs. Eighteen waves of data are available. Due to changes in the measurement instrument in Wave 1, the subjective well-being scores are higher in Wave 1 than those in other waves [Rose (1999)]. We accordingly drop Wave 1 from our analysis and use the data from Wave 2 to Wave 18. Our analysis focuses on the working population only, because the day-of-the-week patterns are more prevalent for the employed.

The individual-level job satisfaction in the BHPS dataset is reported based on a seven-point scale ranging from 1 (not satisfied at all) to 7 (completely satisfied). On each interview, the employed workers are asked to rate their job satisfaction levels regarding the promotion prospects, total income, relationship with boss, job security, able to use their initiatives in the work, the actual work itself, and hours worked. The last question about job satisfaction is “Overall, how satisfied or dissatisfied are you with your present job?”, which is again measured on the 1–7 scale and named the “overall job satisfaction.” This is a direct measure of individuals’ utility derived from their current job [Clark and Oswald (1996)]. We use this overall measure in our analysis.

Happiness/psychological well-being is derived from the General Health Questionnaire (GHQ) in the BHPS. The GHQ is widely used in the United Kingdom as a self-completion assessment measure of minor psychiatric morbidity [Goldberg and Williams (1988), McCabe, Thomas, Brazier, and Coleman (1996)]. This is a reliable indicator of mental distress [Argyle (2001)] and has been used extensively in the medical literature [Goldberg (1972), Goldberg (1978)]. The GHQ measures whether a respondent suffers from a health problem related to anxiety or depression (available at all waves) and overall life satisfaction scores (from wave 6 onwards). The following questions have been asked in the GHQ to measure happiness/psychological well-being. Have you recently:

1. Been able to concentrate on whatever you are doing?
2. Lost much sleep over worry?
3. Felt that you are playing a useful part in things?
4. Felt capable of making decisions about things?
5. Felt constantly under strain?
6. Felt you couldn’t overcome your difficulties?
7. Been able to enjoy your normal day to day activities?
8. Been able to face up to your problems?
9. Been feeling unhappy and depressed?
10. Been losing confidence in yourself?
11. Been thinking of yourself as a worthless person?

12. Been feeling reasonably happy all things considered?

Answers are coded on a four-point scale: from "Disagree strongly" (coded 1) to "Agree strongly" (coded 4). The questions 1, 3, 4, 7, 8, and 12 are coded in the reverse order. The literature typically brings these scores together to provide an aggregate GHQ mental distress score. This final aggregate measure ranges from 12 to 48 [Taylor (2006), Hu, Stewart-Brown, Twigg, and Weich (2007)]. This approach is known as the "Likert Scale". Low scores correspond to low levels of stress/depression (i.e., high feelings of well-being and happiness). We follow this convention and construct our happiness measure consistent with the Likert scale. We focus on this general happiness score in our empirical analysis. It will perhaps be useful to check internal consistency and test-retest reliability of this measure within our sample. To test internal consistency, we calculate the Cronbach's alpha, which is 0.88 for the general happiness measure and between 0.85-0.89 for each of the twelve items listed above. This suggests that the GHQ measures we use are internally consistent. The test-retest reliability scores—which we calculate both through the canonical correlation coefficient and intra-class correlation coefficient based on a mixed-effects linear model—range in the interval 0.52-0.75, which means that the GHQ measure has a reasonably reliable correlation between the test and retest for an annual survey. All the coefficient are significant at 1 percent level.

For the individual- and job-related characteristics, we follow the recent studies using the BHPS and control for gender, age, age-squared, education levels, preferences over working hours, types of contract, size of establishment, promotion opportunities, union membership, and health status [Taylor (2006)]. We collapse the education-levels into seven broad groups as follows: *higher degree* refers to postgraduate education, *first degree* refers to college education, *'A'-level*, *'O'-level* and *other higher qualification* refer to high school graduates of different types (consistent with the UK education system), *vocational* *qualification* refers to teaching, nursing, commercial, apprenticeship, and the certificate of secondary education (CSE), and, finally, the ones with *no qualification*. We also construct a dummy variable ("Income") for earnings. It is equal to 1 if the worker earns more than the median level of earnings in her reference group (in the corresponding wave) and it is equal to 0 otherwise. The reference groups are simply the region-industry combinations, in which the individuals can potentially interact [see Tumen and Zeydanli (2014) for more details on the construction of the reference groups]. We construct such a variable to control for the income-comparison.

Table (3.1) presents the summary statistics of the data that we use in our analysis. In order to be included into our sample, the respondent must be employed and have reported an overall job satisfaction score or a general happiness score. The mean age of the respondents is around 39. Among the 69,000 observations, 50% are male, 56% are married, 33% are never married, 2.9% have higher degree, 12.3% have first degree, another 13.2% have 'A'-level degree, 21.2% have 'O'-level degree, 26.2% have other higher qualifications, 11.6% have vocational qualifications, and the remaining 12.7% have no qualifications. 2.9% and 1.7% have temporary and fixed term contracts, respectively. 17% work in the public sector and 69.1% work in a company of size 200 workers or smaller. 23.9% are union members. 8% prefer to work more hours and 31.4% prefer to work fewer hours. 25% report their health to be very good, whereas 15.9% report to be satisfactory. 52.4% earn above the median monthly income in their respective reference groups. The mean overall job satisfaction rate is 5.38 out of 7, with a standard deviation of 1.296. The mean general happiness score is 22.75 out of 48, with a standard deviation of 5.073. Notice that there are two dummy variables in the table labeled Fri/Sat and Sun/Mon. Fri/Sat is equal to 1 if the interview is conducted on a Friday or Saturday and 0 otherwise. Sun/Mon is equal to 1 if the interview is conducted on a Sunday or Monday and 0 otherwise. 18.9% of the workers in our sample are interviewed on Friday or Saturday, while 24.7% on Sunday or Monday. All the means and the standard deviations reported in Table (3.1) are calculated using the BHPS frequency weights.

3.3 Econometric Framework

The econometric framework we use is a standard random-utility specification in combination with a version of the two-sector Roy model [Roy (1951)].[4] Suppose that the survey respondents can choose whether to respond in some certain days of the week ($D = 1$) versus the remaining days ($D = 0$). For our job satisfaction analysis, $D = 1$ refers to responding the survey on a Friday or Saturday and $D = 0$ refers to responding on the remaining days of the week. Similarly, for happiness, $D = 1$ refers to responding the survey on a Sunday or Monday and $D = 0$ refers to responding on the remaining days of the week. For expositional simplicity, we will mention only $D = 1$ or $D = 0$ without a further reference to the days associated with these choices.

Following the conventional random utility analysis embedded into the binary discrete-choice setup, the equations motivating the individual's choice of $D = 1$ versus $D = 0$ can be written as follows:

$$U_0 = \boldsymbol{Z}\boldsymbol{\alpha}_0' + \nu_0, \tag{3.1}$$

$$U_1 = \boldsymbol{Z}\boldsymbol{\alpha}_1' + \nu_1, \tag{3.2}$$

where $\boldsymbol{Z}$ is a row-vector of observed covariates. This is the standard additive random utility specification, where $\boldsymbol{\alpha}_0'$ and $\boldsymbol{\alpha}_1'$ are the deterministic components, and ν_0 and ν_1 are the random components.

[4] See also Heckman and Honore (1990).

To rationalize the individual's choice of D, we presume an index function

$$I = U_1 - U_0, \tag{3.3}$$

which can be rewritten, after plugging in the random utility equations, as

$$I = \boldsymbol{Z\gamma}' + \eta, \tag{3.4}$$

where $\boldsymbol{\gamma} = \boldsymbol{\alpha}_1 - \boldsymbol{\alpha}_0$ and $\eta = \nu_1 - \nu_0$. The key consideration is that the econometrician observes the subjective (or self-reported) well-being response Y_1 if $I \geq 0$ and he observes Y_0 if $I < 0$. The intuition is as follows. For a moment, let's consider the job satisfaction example. The interviewee takes the interview on a Friday or Saturday ($D = 1$) rather than the other days if she receives higher utility from doing so. This higher utility (i.e., $U_1 > U_0$) is translated into the expression $I \geq 0$. As a result, Y_1 is observed. The utilities are not observed; but, what the econometrician observes are a choice and an associated well-being outcome. The observed subjective well-being outcome, in this setup, is

$$Y = (1 - D)Y_0 + DY_1, \tag{3.5}$$

which means that $Y = Y_0$ if $D = 0$ and $Y = Y_1$ if $D = 1$. Y_1 is observed when $U_1 \geq U_0$ and Y_0 is observed otherwise. The main lesson that this formulation communicates is the following. The day of the week on which the interviewee takes the interview is a matter of choice. There are both observed and unobserved factors that may be affecting this choice. Accounting for unobservables may change the results reported in the literature. This formulation aims at explicitly controlling for unobserved determinants of the day of the week.

To map this formulation to data, we formulate two outcome equations:

$$Y_0 = \boldsymbol{X}\boldsymbol{\beta}_0' + \epsilon_0, \tag{3.6}$$

$$Y_1 = \boldsymbol{X}\boldsymbol{\beta}_1' + \epsilon_1, \tag{3.7}$$

where $\boldsymbol{X}$ is a row-vector of observed covariates. We assume that $(\boldsymbol{X}, \boldsymbol{Z}) \perp\!\!\!\perp (\eta, \epsilon_1, \epsilon_0)$, where $\perp\!\!\!\perp$ denotes statistical independence. We also assume that the error terms are jointly normally distributed as $(\eta, \epsilon_1, \epsilon_0) \sim \mathcal{N}(\mathbf{0}, \boldsymbol{\Sigma})$, where $\boldsymbol{\Sigma}$ is the covariance matrix and can be written as

$$\boldsymbol{\Sigma} = \begin{bmatrix} \sigma_{\eta\eta} & \sigma_{\eta 1} & \sigma_{\eta 0} \\ \sigma_{\eta 1} & \sigma_{11} & \sigma_{10} \\ \sigma_{\eta 0} & \sigma_{10} & \sigma_{00} \end{bmatrix}. \tag{3.8}$$

Note that from $\eta = \epsilon_1 - \epsilon_0$, it is easy to show that $\sigma_{\eta\eta} = \sigma_{11} + \sigma_{00} - 2\sigma_{10}$, $\sigma_{\eta 1} = \sigma_{11} - \sigma_{10}$, and $\sigma_{\eta 0} = \sigma_{10} - \sigma_{00}$.

As we explain above, $D = \mathbb{1}(I \geq 0)$, where $\mathbb{1}$ stands for an indicator function. From data on Y, D, and $(\boldsymbol{X}, \boldsymbol{Z})$, the following quantities can be obtained:

$$\mathbb{P}[D = 1|\boldsymbol{Z}], \ \ \mathbb{E}[Y|D = 1, \boldsymbol{X}, \boldsymbol{Z}], \ \text{ and } \ \mathbb{E}[Y|D = 0, \boldsymbol{X}, \boldsymbol{Z}].$$

One key issue is the distinction between $\boldsymbol{Z}$ and $\boldsymbol{X}$. For identification purposes, we assume that these two data vectors overlap except one extra variable in $\boldsymbol{Z}$; that is, $\dim(\boldsymbol{Z}) = \dim(\boldsymbol{X}) + 1$, where the notation "dim" describes the dimension of a data vector. In other words, we need an extra variable that affects the choice of the agent, but does not affect the outcome of interest. This is known in the literature as an "exclusion restriction" (or an instrument) that secures identification in selection-correction models. See Section 3.3.3 for a comprehensive discussion of this issue as well as the details of the specific exclusion restriction that we use in this paper.

3.3.1 Selection Correction

We start with the following Probit regression, which is the typical first step in a selection-correction procedure:

$$\begin{aligned}
\mathbb{P}\big[D=1\big|\boldsymbol{Z}=\boldsymbol{z}\big] &= \mathbb{P}\big[\boldsymbol{Z}\boldsymbol{\gamma}'+\eta\geq 0\big|\boldsymbol{Z}=\boldsymbol{z}\big] \\
&= \mathbb{P}\big[\boldsymbol{z}\boldsymbol{\gamma}'+\eta\geq 0\big] \\
&= \mathbb{P}\left[\frac{\eta}{\sigma_\eta}\geq -\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right] \\
&= \Phi\left(\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right). \qquad (3.9)
\end{aligned}$$

This probit equation identifies $\boldsymbol{\gamma}/\sigma_\eta$, where $\sigma_\eta=\sqrt{\sigma_{\eta\eta}}$. Now we consider the regression equations related to the two outcome equations. The first outcome equation gives

$$\begin{aligned}
\mathbb{E}\big[Y\big|D=1,\boldsymbol{X}=\boldsymbol{x},\boldsymbol{Z}=\boldsymbol{z}\big] &= \mathbb{E}\big[Y_1\big|\boldsymbol{X}=\boldsymbol{x},\boldsymbol{Z}=\boldsymbol{z}\big] \\
&= \boldsymbol{x}\boldsymbol{\beta}_1'+\mathbb{E}\big[\epsilon_1\big|\boldsymbol{z}\boldsymbol{\gamma}'+\eta\geq 0\big] \\
&= \boldsymbol{x}\boldsymbol{\beta}_1'+\frac{\sigma_{\eta 1}}{\sigma_\eta}\lambda\left(-\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right) \qquad (3.10)
\end{aligned}$$

and the second outcome equation gives

$$\begin{aligned}
\mathbb{E}[Y|D=0,\boldsymbol{X}=\boldsymbol{x},\boldsymbol{Z}=\boldsymbol{z}] &= \mathbb{E}[Y_0|\boldsymbol{X}=\boldsymbol{x},\boldsymbol{Z}=\boldsymbol{z}] \\
&= \boldsymbol{x}\boldsymbol{\beta}_0'+\mathbb{E}[\epsilon_0|\boldsymbol{z}\boldsymbol{\gamma}'+\eta<0] \\
&= \boldsymbol{x}\boldsymbol{\beta}_0'-\frac{\sigma_{\eta 0}}{\sigma_\eta}\lambda\left(\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right), \qquad (3.11)
\end{aligned}$$

where $\lambda(\cdot)$ is the inverse Mills ratio and, as a general rule, $\lambda(c)=\phi(c)/\Phi(-c)$.

From the probit regression in (3.9), we already know the parameter $\boldsymbol{\gamma}/\sigma_\eta$. Therefore, we can form $\lambda\left(-\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right)$ and $\lambda\left(\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right)$. Equations (3.10) and (3.11) suggest that we can run regressions of Y_1 on $\boldsymbol{X}$ and $\lambda\left(-\frac{\boldsymbol{z}\boldsymbol{\gamma}'}{\sigma_\eta}\right)$, and of Y_0 on

$\boldsymbol{X}$ and $\lambda\left(\frac{\boldsymbol{z\gamma}'}{\sigma_\eta}\right)$ to identify $\boldsymbol{\beta}_0$, $\boldsymbol{\beta}_1$, $\sigma_{\eta 0}/\sigma_\eta$, and $\sigma_{\eta 1}/\sigma_\eta$.[5]

3.3.2 Treatment Effects

In our context, "treatment" refers to taking the interview on a Friday or Saturday for job satisfaction analysis and Sunday or Monday for happiness analysis (i.e., $D = 1$). Obtaining the treatment effect estimates would be useful for our analysis, since it will enhance our understanding of the existence, extent, and the sources of the selection structure. Calculation of the treatment effects are simple and straightforward after obtaining the bias corrected estimates described in the previous subsection. The most commonly sought treatment effect parameter is the Average Treatment Effect (ATE). It can simply be formulated as

$$\begin{aligned} \text{ATE}(\boldsymbol{x}) &= \mathbb{E}[Y_1 - Y_0 | \boldsymbol{X} = \boldsymbol{x}] \\ &= \boldsymbol{x}(\boldsymbol{\beta}_1' - \boldsymbol{\beta}_0'). \end{aligned} \tag{3.12}$$

This can be interpreted as the effect of randomly assigning $D = 1$ to everyone with $\boldsymbol{X} = \boldsymbol{x}$. The main problem with this parameter is analogous to the central question that we address in this paper; that is, it may not reflect a true causal effect of $D = 1$ versus $D = 0$ on the subjects, because the ones who choose $D = 1$ maybe systematically different from the ones who choose $D = 0$.[6] This difference leads the evaluation of the outcome at the counterfactual states to be biased.

The other two treatment effect parameters that we calculate in this study are the treatment on the treated (TT) and the treatment on the untreated

[5] Identification of the other parameters is also possible. But, in this paper, we are not interested in the magnitudes of the rest of the parameters. See Heckman and Honore (1990) for the details. See also Heckman and Robb (1985) and Heckman and Vytlacil (2007a,b).

[6] Remember that in our case $D = 1$ refers to taking the interview on a Friday or Saturday versus the remaining days for the job satisfaction analysis and on a Sunday or Monday versus the remaining days for the happiness analysis.

(TUT). These parameters can nicely be formulated as a function of the control functions estimated during the implementation of the selection-correction procedure [see Heckman and Vytlacil (2007a,b) for details]. The parameter TT can be formulated as

$$\begin{aligned}\mathrm{TT}(\boldsymbol{x}, p_{\boldsymbol{z}}) &= \mathbb{E}[Y_1 - Y_0 | D = 1, \boldsymbol{X} = \boldsymbol{x}, \boldsymbol{Z} = \boldsymbol{z}] \\ &= \boldsymbol{x}(\boldsymbol{\beta}_1' - \boldsymbol{\beta}_0') + \frac{\sigma_{\eta 1} - \sigma_{\eta 0}}{\sigma_\eta} \frac{\phi(\Phi^{-1}(p_{\boldsymbol{z}}))}{p_{\boldsymbol{z}}},\end{aligned} \tag{3.13}$$

while TUT can be formulated as

$$\begin{aligned}\mathrm{TUT}(\boldsymbol{x}, p_{\boldsymbol{z}}) &= \mathbb{E}[Y_1 - Y_0 | D = 0, \boldsymbol{X} = \boldsymbol{x}, \boldsymbol{Z} = \boldsymbol{z}] \\ &= \boldsymbol{x}(\boldsymbol{\beta}_1' - \boldsymbol{\beta}_0') + \frac{\sigma_{\eta 0} - \sigma_{\eta 1}}{\sigma_\eta} \frac{\phi(\Phi^{-1}(p_{\boldsymbol{z}}))}{1 - p_{\boldsymbol{z}}},\end{aligned} \tag{3.14}$$

where $p_{\boldsymbol{z}}$ refers to the propensity score estimated in the first stage probit regression. The average TT is the average gain for those who sort into treatment compared to what the average person would gain. It oversamples the unobserved characteristics that lead to selectivity for those individuals who are more likely to choose $D = 1$. In other words, it calculates the net effect between those who actually participate and those who do not, as if they had given the chance to revert their choice of $D = 0$ into $D = 1$. A symmetric definition can be provided for TUT, oversamples the unobserved characteristics that lead to selectivity for those individuals who are less likely to choose $D = 1$.

For the purposes of this paper, we are interested in the "averages" of these three treatment effect parameters. In other words, the estimates reported at the end of the paper are the parameter estimates integrated over the entire horizon of $\boldsymbol{x}$ and $\boldsymbol{z}$ in our sample. It is also possible to report the distribution of these treatment effects over the sample space. But, we report only the means to keep the paper as compact as possible.

Notice that when the coefficient of the inverse Mills ratio calculated at the second stage is zero, then the TT and TUT collapses into ATE. This is the case with no selectivity. When there is positive sorting into the treatment state (as in our case), on the other hand, the econometrician would find TT $>$ ATE $>$ TUT. Moreover, it is easy to verify that ATE $= p \times$ TT $+ (1-p) \times$ TUT. In Section 3.4, we use these formulas and calculate the treatment effect parameters for both the job satisfaction and happiness scores.

3.3.3 The Exclusion Restriction

There are two traditional ways through which the Heckman selection-correction method can be applied. The first one is the existence of an additional variable in the selection equation, which does not affect the outcome of interest. This is called the "exclusion restriction" (or "instrument") and it secures identification of bias-corrected estimates. The second one is to use the nonlinearity inherent in the inverse Mills ratio. In this case, identification solely comes from the normality assumption. The latter is disadvantageous for two reasons; (i) self-selection may not be originating from a normally distributed process [Little and Rubin (1987)] and (ii) the inverse Mills ratio may still be highly collinear with the other regressors in the outcome equation [Leung and Yu (1996)]. A potential disadvantage of the exclusion restriction approach is that there is no natural guide to specify a variable that affects the choice but does not affect the outcome; moreover, a wrongful implementation of the restriction may be harmful [Manning, Duan, and Rogers (1987)]. Nevertheless, the main consensus is that, using an appropriate exclusion restriction, if there exists one, will secure a more convincing identification of the selection-corrected estimates. Fortunately, the BHPS dataset allows us to construct a sensible exclusion restriction for our analysis.

We use the interviewer ID as an exclusion restriction. More precisely, we rely on the identifying assumption that who the interviewer is a determinant of when the interviewee takes the survey, but it is not a determinant of the survey outcome (i.e., happiness and job satisfaction). Figures (3.1) and (3.2) plot the tendencies of the interviewers in terms of timing. To be concrete, Figure (3.1) presents the distribution of the interviewers' probabilities of conducting the survey on a Friday or Saturday. Similarly, Figure (3.1) describes the distribution of the interviewers' probabilities of conducting the survey on a Sunday or Monday. For example, a value of 0.4 read on the horizontal axis should be interpreted as a 40% of the interviews conducted by that particular interviewer are on a Friday or Saturday. Clearly, some interviewers are more likely to conduct the interview of certain days.

The validity of this exclusion restriction is justified in the empirical analysis presented by Taylor (2006). He shows that the interviewer ID likely affects the day on which the interview is conducted; but, it does not affect the outcome (i.e., the subjective well-being score). We follow this suggestion and use interviewer ID as an exclusion restriction in our selection-correction exercise.

To construct the variable that we use as the exclusion restriction, we determine the mean values in these two distributions. We generate a binary variable taking the value 1 if the interviewer's probability is greater than the mean and 0 otherwise. This new dummy variable characterizes if the interviewer is more likely to conduct the interview on a Friday or Saturday (Sunday or Monday) than the average tendency in the job satisfaction (happiness) analysis. The mean tendency to conduct the interview on a Friday or Saturday is 0.189 for job satisfaction and the corresponding mean tendency to conduct the interview on a Sunday or Monday for happiness is around 0.24. Table (3.5) documents that this binary variable (i.e., interviewer dummy) is a relevant determinant of the day of interview. Intuitively, who the interviewer is should not be a systematic determinant of well-being. As a result, we use this dummy variable as an exclusion restriction in our selection-correction exercise.

3.4 Results and Discussion

In this section, we document the empirical results and provide an extensive discussion of the main implications of our analysis. We start with a simple observation. Fridays and Saturdays are the days on which the self-reported job satisfaction scores are higher, on average, than the scores reported on the other days. Moreover, Sundays and Mondays are the days on which the self-reported happiness scores are lower, on average, than the scores reported on the remaining days of the week. These raw patterns are best observed from the results of an OLS regression of the associated well-being score on the day dummies. Tables (3.2) and (3.3) document these patterns.

The second step is to see whether including observed characteristics into these regressions changes these results or not. We include a comprehensive set of regressors for both worker- and job-related characteristics. The worker-related regressors include age as a quadratic polynomial and dummy variables for gender, marital status, education, health, region, and the year of interview. The job-related regressors include dummy variables for job contractual status, permanency of job, promotion opportunities, union membership status, public/private sector job, firm size, preference for work hours, relative income, and industry.[7] We perform two separate regressions for job satisfaction and happiness controlling for these variables as well as the day-of-the-week dummies. We find that the results of the simple regressions described above are reinstated; that is, on average, job satisfaction scores are higher for those interviewed on Fridays or Saturdays and happiness scores are lower for those interviewed on Sundays or Mondays. These results are in line with the day-of-the-week

[7] Table (3.1) presents the basic summary statistics for these variables as well as the outcome variables.

patterns documented by the main papers in the related literature.[8] See Table (3.4) for the results.

We investigate if there is any sorting on unobservables that can potentially bias these day of the week patterns. If self-selection is in effect, then individuals with certain unobserved characteristics tend to respond the survey on certian days of the week. For example, those who respond the survey on a Friday or Saturday may be the ones who are the most satisfied with their jobs. These individuals may have a strong motivation to work hard during the week and the only available time for them to respond may be a Friday afternoon or a Saturday. In this example, "motivation" is the unobserved variable. There may also be other unobserved factors which are also relevant for this example. For happiness, those who respond the survey on a Sunday or Monday may be the ones who are the most unhappy ones with their jobs or lives in general due to some unobserved factors. These individuals may be, say, the least conscientious[9] ones, therefore they are the ones who are more likely to express their unhappiness at the end of a weekend vacation or at the beginning of a busy week. One can easily extend these examples.

If selection is a concern, then the differences in days, in terms of subjective well-being outcomes, may be driven by these unobserved individual-level heterogeneity components. In other words, the day of the week patterns can be explained by non-random sorting on unobservables if self-selection is strong. To test this hypothesis, we perform a simple selection-correction procedure motivated by a combination of the Roy model with a standard random utility specification described in Section 3.3. As we discuss in Section 3.3.3, the interviewer ID is used to construct an exclusion restriction to secure identification.

[8]See, for example, Taylor (2006), Akay and Martinsson (2009), and Helliwell and Wang (2013).

[9]Conscientiousness is one of the big-five personality traits that constitute an individual's non-cognitive skills. See Borghans, Duckworth, Heckman, and ter Weel (2008) for an extensive description of these concepts.

Table (3.5) documents the results our first-stage probit estimations. Tables (3.6) and (3.7) report the second-stage regressions, in which we use the inverse Mills ratios constructed from the first-stage as regressors.

Our results reveal that selectivity is very strong; that is, individuals sort into the days of the week based on their unobserved characteristics that affect the outcomes. Back of the envelope calculations (i.e., averaging the inverse Mills ratio and multiplying this average with the estimated coefficient) yield the result that almost all of the difference between Friday/Saturday ($D = 1$) and the remaining days ($D = 0$) disappear after controlling for selectivity. Similar calculations show that, after controlling for selectivity, Sunday and Monday ($D = 1$) are actually happier days than the other days of the week ($D = 0$).

We perform a further investigation of these selectivity patterns using the treatment effect parameters described in Section 3.3.2. Tables (3.8), (3.9), (3.10) and Figures (3.3), (3.4) document these estimates. The existence of selectivity is confirmed from the result that TT > ATE > TUT. The treatment on the treated parameter is quite high, strongly supporting the "non-random sorting on unobservables" idea.

We further show that, for job satisfaction, the estimated treatment effects are higher among males, non-married workers, workers with permanent jobs, public sector workers, workers in large firms, union members, workers with good health, workers who prefer to work less, workers with higher relative income, workers with higher education, and middle-aged workers. These patterns are important, because the existence of significant selectivity signals that the OLS estimates of the coefficients of other observed covariates are biased. There is a consensus in the empirical job satisfaction literature using BHPS—see, e.g., Taylor (2006)—that, on average, females, married workers, and workers with low education levels are more satisfied with their jobs. Our estimates show that these results are biased. For example, married workers are known to be more satisfied jobwise. Our raw OLS estimates reported in Table (3.4) reads a coefficient of 0.066 for marriage. The selection corrected estimates yield coefficients of 0.025 for the $D = 1$ sector and 0.075 for the $D = 0$ sector. In terms of our results, this suggests that workers whose unobserved characteristics lead to relatively lower job satisfaction ($D = 0$) tend to be married and this generates a higher coefficient in the $D = 0$ group versus a much lower coefficient in the $D = 1$ group. The signs of the coefficients have not changed after correcting for selection, but the magnitudes have become much weaker. Another example is for the job satisfaction patterns across age groups [see Figures (3.3) and (3.4)]. The literature reports that—see, e.g., Clark, Oswald, and Warr (1996)—there is a U-shaped relationship between job satisfaction and age.[10] Our findings reveal that the selection bias takes an inverse U-shape over the life course; therefore, the observed U-shaped relationship between well-being and age is likely biased, too. All these patterns are also observed for happiness along similar lines.

The main practical implication of this study is that the observed day-of-the-week effects are mostly due to compositional shifts rather than behavioral changes. We show that the compositional effects are driven by heterogeneity in unobserved factors that diffuse into individuals' choice of the interview date. This result does not mean that psychological factors have no effect on well-being. It rather suggests that the observed day-of-the-week patterns should not be interpreted as direct evidence of the link between "mood" and well-being. Uncovering the details of the unobserved factors driving compositional shifts is an interesting topic for future research, but it is out of the scope of this paper.

We conduct our analysis with the BHPS, which is a representative dataset for the United Kingdom. This means that both the observed day-of-the-week pat-

[10] See, for example, Blanchflower and Oswald (2008) for similar findings for happiness.

terns and the results of the selection-correction exercise should be interpreted taking the British cultural norms as the benchmark. Depending on the country, norms, and even religious beliefs, the observed day- of-the-week patterns may change. For example, Monday is a major "blue" day in the United Kingdom, while Sunday is shown to be "blue" in Germany [Akay and Martinsson (2009)]. In North America, on the other hand, Sunday is often reported as a happy day [Helliwell and Wang (2013)]. The results may change further in, say, Muslim or Jewish societies. Although the observed day-of-the-week patterns tend to change across cultures, we believe that our analysis has broader implications that can be relevant for other countries, regions, and cultures. In some sense, our results imply that the cross-cultural differences in the observed day-of-the-week patterns will tend to disappear after correcting for selectivity. But, it may well the case that the unobserved characteristics that lead to selectivity can also be based on cultural differences, social norms, differences in working hours, worker motivation, expectations, etc. Further empirical research is needed to test the validity of these cross-cultural concerns.

The BHPS is not the only dataset on which our procedures can be implemented. The same empirical exercise can be performed for other countries, where micro-level subjective well-being datasets are available with proper "date of interview" information. For example, a similar analysis may also be conducted using the German Socio-Economic Panel (GSOEP) dataset. Another dataset that can be used is the Gallup/Healthways U.S. daily poll. However, the same analysis cannot be carried out with datasets like World Values Survey, Euro-barometer, and International Social Survey Programme, because the date of interview is mostly missing in these datasets.

A potential limitation of our analysis is related to the instrument we use in the selection-correction exercise. The instrument—or the exclusion restriction—has to be correlated with the choice of the interview day, but it has to be uncorrelated with the outcome, i.e., the subjective well-being score. In other words, the instrument has to be placed into the choice equation, but excluded from the outcome equation to guarantee identification. To satisfy these requirements, we use the interviewer ID number as our exclusion restriction. We argue that who the interviewer is can affect the interview day, because some interviewers may be more likely to work at weekends than the others. However, we also argue that the interviewer ID has very little or no effect on the interviewee's responses. The limitation may apply at this point: if the interviewee's response is systematically affected by the interviewer ID, then this logic would not work. We perform several robustness checks to question the relevance of this concern. We find that some interviewers are indeed much more inclined to conduct interviews at weekends than the others. We also find that the interviewer dummies are mostly insignificant in the regression of the well-being score on all the explanatory variables and the interviewer ID's. This provides suggestive evidence that the interviewer ID might be a valid instrument.

3.5 Concluding Remarks

In this paper, we investigate whether the day-of-the-week effect estimates reported in the empirical subjective well-being literature suffer from selectivity bias. We use the BHPS dataset to answer this question. Our answer is yes; that is, we show that the observed day-of-the-week patterns is a by product of non-random sorting of individuals into the days of the week. More precisely, we show that individuals who take the BHPS interview on a Friday or Saturday—the days on which the self-reported job satisfaction score is the highest—are selectively different in terms of their unobserved characteristics from the ones interviewed on the remaining days. Similarly, the individuals who take the BHPS interview on a Sunday or Monday—the days on which the self-reported happiness score is the lowest—are selectively different in terms of their unobserved characteristics from the ones interviewed on the remaining days. We conclude that the argument supporting the existence of weekly cycles in individual utility is not as strong as the literature suggests. We also discuss the potential channels through which the self-selection process operates.

The previous literature argues that, everything else constant, the individual well-being is lower in certain days of the week than the remaining days. This is generally interpreted as an evidence supporting the view that individuals assess their well-being at any given moment over time. Subjective well-being measures are often used to proxy individual-level utility (or preferences), which is the main building block of the theory of economic decisions. Thus, if well-being is an "objective" motivating economic choices, then the decisions made on Sundays would be different than those made on, say, Wednesdays. This implies that behavioral changes can mostly be attributed to psychological factors. However, this is strictly against the neoclassical economic theory, which is built on the basic idea that preferences should not change often (i.e., they are stable). Our findings provide evidence that the existence of weekly cycles in individual well-being may not be as relevant as the literature documents. Our results reveal that interpreting the observed day-to-day differences in the average subjective well-being scores as mood fluctuations might be incorrect. We *do not* say that preferences are not affected by psychological motives. We say that ruling out the neoclassical economic theory based on the uncorrected day-of-the-week patterns might produce misleading results.

We provide an alternative explanation for the observed day-of-the-week patterns in subjective well-being scores: the composition of survey respondents in terms of their unobserved characteristics changes across the days of the week on a non-random basis. We argue that these compositional shifts have a potential to be falsely interpreted as mood fluctuations. That said, we do not totally rule out the state-dependent nature of utility. Utility may be changing across states if these states reflect some fundamental feature of individual utility; such as employment status, marital status, etc. We rather argue that day-to-day shifts in agents' valuation of economic objects do not have strong empirical basis, when selectivity is controlled for.

Table 3.1: SUMMARY STATISTICS—BHPS (1992-2009)

Summary Statistics				
Variable	Mean	Standard Deviation	Min.	Max.
Job satisfaction	5.383	1.296	1	7
Happiness	22.748	5.073	12	48
Male	0.497	0.500	0	1
Age	38.749	12.900	16	85
Married	0.563	0.496	0	1
Never married	0.328	0.470	0	1
Higher degree	0.029	0.168	0	1
First degree	0.123	0.328	0	1
'A'-levels	0.132	0.339	0	1
'O'-levels	0.212	0.409	0	1
Other higher qual.	0.262	0.440	0	1
Vocational qual.	0.116	0.320	0	1
No degree	0.127	0.332	0	1
Temporary worker	0.029	0.167	0	1
Fixed-term contract	0.017	0.129	0	1
Public sector worker	0.170	0.376	0	1
Small employer	0.691	0.462	0	1
Promotion opp.	0.405	0.491	0	1
Union member	0.239	0.426	0	1
Health very good	0.250	0.433	0	1
Health very satisfactory	0.159	0.366	0	1
Prefers to work more	0.080	0.271	0	1
Prefers to work less	0.314	0.464	0	1
Income	0.524	0.499	0	1
Fri/Sat	0.189	0.392	0	1
Sun/Mon	0.247	0.431	0	1

Notes: This table roughly summarizes the data we use. We focus on employed individuals in the BHPS data covering the period 1992–2009. Appropriate sampling weights are used.

Table 3.2: Day orderings

Dependent variable	Job Satisfaction		Happiness	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Friday	0.0340***	(0.0020)	-0.0405***	(0.0076)
Saturday	0.0221***	(0.0022)	-0.0208**	(0.0086)
Sunday	-0.0041	(0.0027)	0.0709***	(0.0106)
Monday	0.0032**	(0.0017)	0.1008***	(0.0066)
Tuesday	0.0191***	(0.0017)	0.0683***	(0.0065)
Wednesday	-0.0072***	(0.0017)	-0.0095	(0.0065)
Thursday	Omitted		Omitted	
Constant	5.367***	(0.0012)	22.719***	(0.0048)
# of observations	68,773		68,504	

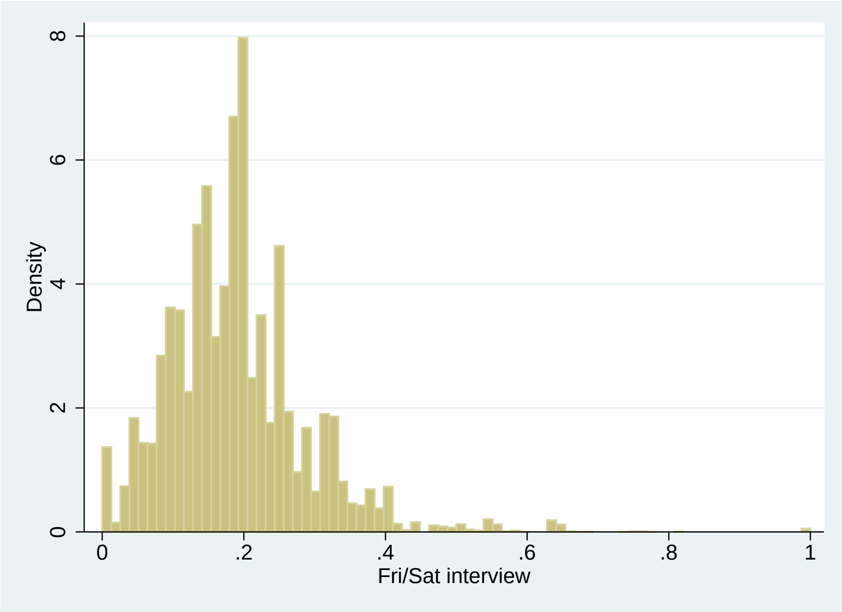
Notes: This table presents the results of an OLS regression of the subjective well-being score on the days of the week. Thursday is the omitted dummy variable; that is, the results should be read with respect to Thursday. For job satisfaction, the raw ordering of the days are as Friday, Saturday, Tuesday, Monday, Thursday, Sunday, and Wednesday, from the most satisfied to the least satisfied day. For happiness, the raw ordering of the days are as Friday, Saturday, Wednesday, Thursday, Tuesday, Sunday, and Monday, from the best to the worst day. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Table 3.3: Bunching the days

Dependent variable	**Job Satisfaction**		**Happiness**	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Fri/Sat	0.0255***	(0.0013)	-	-
Sun/Mon	-	-	0.0879***	(0.0047)
Constant	5.371***	(0.0006)	22.726***	(0.0023)
# of observations	68,773		68,504	

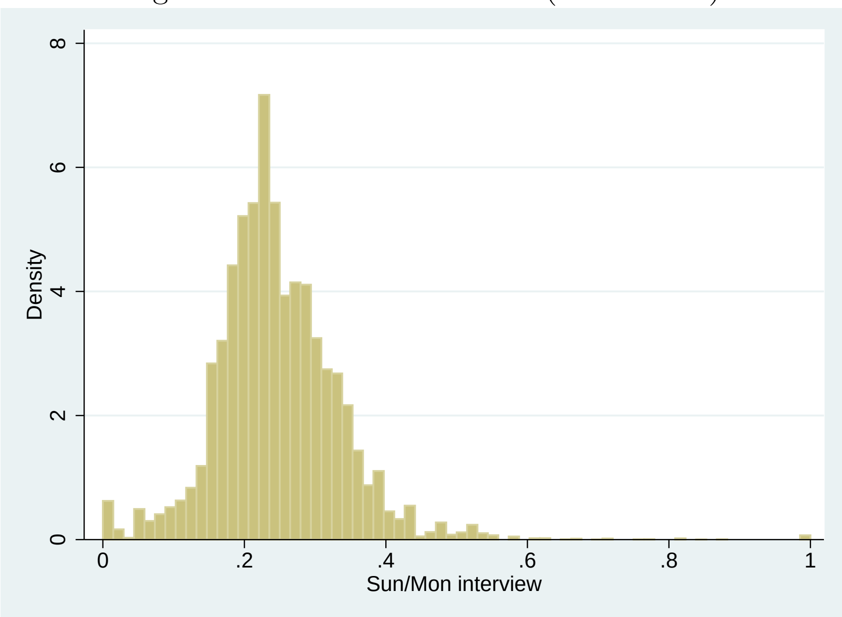
Notes: This table repeats the exercise above by regressing the job satisfaction (happiness) score on the Fri/Sat (Sun/Mon) dummy. The Fri/Sat (Sun/Mon) dummy indicates if the interview is conducted on a Friday or Saturday (Sunday or Monday). Individuals interviewed on a Friday or Saturday report, on average, higher job satisfaction scores than the ones interviewed on the remaining days. Similarly, Individuals interviewed on a Sunday or Monday report, on average, lower happiness scores than the ones interviewed on the remaining days. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Figure 3.1: Interviewer ID (job satisfaction)



Notes: This figure presents the distributional features of the interviewer ID variable that we use at the probit regression for job satisfaction score. The horizontal axis describes the probability for a specific interviewer to conduct the interview on a Friday or Saturday. For example, a value of 0.4 for interviewer j means that the interviewer j conducted 40% of his/her interviews on a Friday or Saturday.

Figure 3.2: Interviewer ID (happiness)



Notes: This figure presents the distributional features of the interviewer ID variable that we use at the probit regression for the general happiness score. The horizontal axis describes the probability for a specific interviewer to conduct the interview on a Sunday or Monday. For example, a value of 0.4 for interviewer j means that the interviewer j conducted 40% of his/her interviews on a Sunday or Monday.

Table 3.4: Day patterns conditional on observed variation.

Dependent var.	Job Satisfaction		Happiness	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Fri/Sat	0.032***	(0.0012)	-	-
Sun/Mon	-	-	0.097***	(0.0045)
Male	-0.241***	(0.0011)	-1.178***	(0.0044)
Age	-0.038***	(0.0003)	0.205***	(0.0010)
Age^2/100	0.052***	(0.0003)	-0.243***	(0.0012)
Married	0.066***	(0.0016)	-0.660***	(0.0072)
Never married	-0.141***	(0.0020)	-0.511***	(0.0088)
Higher degree	-0.133***	(0.0031)	0.354***	(0.0135)
First degree	-0.242***	(0.0022)	0.323***	(0.0086)
'A'-levels	-0.189***	(0.0021)	0.109***	(0.0079)
'O'-levels	-0.088***	(0.0019)	-0.021***	(0.0069)
Other higher qual.	-0.139***	(0.0019)	0.129***	(0.0070)
Vocational qual.	-0.073***	(0.0021)	-0.111***	(0.0078)
Temporary worker	-0.125***	(0.0042)	-0.026**	(0.0122)
Fixed-term contract	-0.052***	(0.0044)	-0.283***	(0.0163)
Public sector worker	-0.001	(0.0015)	-0.012*	(0.0062)
Small employer	0.149***	(0.0011)	-0.010**	(0.0043)
Promotion opp.	0.319***	(0.0011)	-0.577***	(0.0044)
Union member	-0.188***	(0.0013)	0.339***	(0.0052)
Health very good	0.225***	(0.0011)	-1.665***	(0.0043)
Health very satisfactory	-0.166***	(0.0015)	1.387***	(0.0060)
Prefers to work more	-0.241***	(0.0021)	0.685***	(0.0080)
Prefers to work less	-0.518***	(0.0011)	0.831***	(0.0044)
Income	0.063***	(0.0013)	-0.170***	(0.0049)
Year dummies	Yes		Yes	
Industry dummies	Yes		Yes	
Region dummies	Yes		Yes	
Constant	6.21***	(0.0098)	20.162***	(0.0378)
# of observations	68,773		68,504	
R^2	0.0921		0.0813	

Notes: This table repeats the exercise in Table (3.3) by controlling for a comprehensive set of observed worker- and job-related characteristics. The results suggest that the day patterns are even stronger when observed variation is controlled for. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Table 3.5: Probit regression.

Dependent var.	**Fri/Sat**		**Sun/Mon**	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Male	0.029***	(0.0013)	0.009***	(0.0012)
Age	0.017***	(0.0003)	0.002***	(0.0003)
$Age^2/100$	-0.019***	(0.0004)	-0.002***	(0.0003)
Married	-0.045***	(0.0019)	0.008***	(0.0018)
Never married	-0.032***	(0.0023)	0.034***	(0.0023)
Higher degree	0.154***	(0.0038)	0.061***	(0.0036)
First degree	0.112***	(0.0026)	0.040***	(0.0024)
'A'-levels	0.062***	(0.0025)	-0.003	(0.0023)
'O'-levels	0.060***	(0.0022)	0.029***	(0.0020)
Other higher qual.	0.057***	(0.0022)	0.011***	(0.0020)
Vocational qual.	0.050***	(0.0024)	0.025***	(0.0022)
Temporary worker	0.042***	(0.0044)	-0.050***	(0.0034)
Fixed-term contract	-0.010**	(0.0049)	-0.088***	(0.0044)
Public sector worker	0.004**	(0.0018)	-0.039***	(0.0017)
Small employer	0.004***	(0.0013)	0.007***	(0.0012)
Promotion opp.	0.017***	(0.0013)	0.002	(0.0012)
Union member	0.015***	(0.0015)	-0.019***	(0.0015)
Health very good	0.003*	(0.0014)	0.012***	(0.0013)
Health very satisfactory	-0.013***	(0.0016)	0.026***	(0.0016)
Prefers to work more	-0.027***	(0.0023)	0.018***	(0.0021)
Prefers to work less	-0.007***	(0.0013)	0.006***	(0.0012)
Income	0.011***	(0.0015)	0.019***	(0.0014)
Interviewer dummy	0.524***	(0.0004)	0.410***	(0.0011)
Year dummies	Yes		Yes	
Industry dummies	Yes		Yes	
Region dummies	Yes		Yes	
Constant	-1.482***	(0.0118)	-0.904***	(0.0104)
# of observations	68,773		68,504	
Pseudo R^2	0.0380		0.0215	

Notes: This table documents the results of the probit regression of the day selection of the interviewee on a set of observed characteristics and the interviewer dummy. This is the first step of the usual Heckman two-step selection correction procedure. The interviewer dummy takes the value 1 if the interviewer is more likely to conduct the interview—than the average tendency—on a Friday or Saturday (Sunday or Monday) in the job satisfaction (happiness) analysis. It affects the interviewee's choice of the interview day, but it does not affect the outcome. Thus, it can serve as an exclusion restriction in the selection correction analysis. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Table 3.6: Second step (job satisfaction)

Dependent variable: Job Satisfaction				
	(Y_1) Fri/Sat=1		(Y_0) Fri/Sat=0	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Male	-0.204***	(0.0026)	-0.248***	(0.0013)
Age	-0.029***	(0.0007)	-0.038***	(0.0003)
$Age^2/100$	0.041***	(0.0008)	0.053***	(0.0003)
Married	0.025***	(0.0036)	0.075***	(0.0018)
Never married	-0.150***	(0.0044)	-0.141***	(0.0023)
Higher degree	-0.076***	(0.0070)	-0.144***	(0.0035)
First degree	-0.241***	(0.0050)	-0.239***	(0.0025)
'A'-levels	-0.173***	(0.0050)	-0.189***	(0.0023)
'O'-levels	-0.095***	(0.0044)	-0.083***	(0.0021)
Other higher qual.	-0.171***	(0.0044)	-0.129***	(0.0020)
Vocational qual.	-0.093***	(0.0049)	-0.067***	(0.0023)
Temporary worker	-0.196***	(0.0099)	-0.107***	(0.0046)
Fixed-term contract	-0.011	(0.0099)	-0.061***	(0.0049)
Public sector worker	0.014***	(0.0034)	-0.005***	(0.0016)
Small employer	0.122***	(0.0025)	0.156***	(0.0012)
Promotion opp.	0.340***	(0.0025)	0.313***	(0.0012)
Union member	-0.167***	(0.0030)	-0.193***	(0.0014)
Health very good	0.244***	(0.0026)	0.219***	(0.0013)
Health very satisfactory	-0.161***	(0.0033)	-0.168***	(0.0016)
Prefers to work more	-0.286***	(0.0049)	-0.229***	(0.0023)
Prefers to work less	-0.494***	(0.0026)	-0.524***	(0.0013)
Inverse Mills Ratio	0.0415***	(0.0059)	-0.0797***	(0.0054)
Year dummies	Yes		Yes	
Industry dummies	Yes		Yes	
Region dummies	Yes		Yes	
Constant	6.192***	(0.0258)	6.240***	(0.0107)
# of observations	12,901		55,872	
R^2	0.0845		0.0951	

Notes: This table presents the results of the second step OLS regression of the job satisfaction score on a set of observed covariates (excluding the interviewer dummy) and the inverse Mills ratio calculated from the results of the first step probit regression, which are given in Table (3.5). Clearly, the inverse Mills ratios indicate a significant positive-selection into the treatment sector. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Table 3.7: Second step (happiness)

Dependent variable: Happiness				
	(Y_1) Sun/Mon=1		(Y_0) Sun/Mon=0	
Variable	Coefficient	(Standard Error)	Coefficient	(Standard Error)
Male	-1.178***	(0.0089)	-1.173***	(0.0051)
Age	0.217***	(0.0020)	0.201***	(0.0012)
Age^2/100	-0.261***	(0.0024)	-0.237***	(0.0013)
Married	-0.708***	(0.0148)	-0.641***	(0.0082)
Never married	-0.546***	(0.0180)	-0.490***	(0.0101)
Higher degree	0.766***	(0.0274)	0.222***	(0.0155)
First degree	0.318***	(0.0177)	0.336***	(0.0098)
'A'-levels	0.091***	(0.0162)	0.113***	(0.0090)
'O'-levels	0.004	(0.0145)	-0.025***	(0.0079)
Other higher qual.	0.039***	(0.0145)	0.164***	(0.0080)
Vocational qual.	-0.264***	(0.0161)	-0.054***	(0.0089)
Temporary worker	-0.258***	(0.0241)	-0.218***	(0.0186)
Fixed-term contract	-0.524***	(0.0340)	-0.061***	(0.0049)
Public sector worker	0.149***	(0.0130)	-0.072***	(0.0070)
Small employer	0.081***	(0.0088)	-0.039***	(0.0050)
Promotion opp.	-0.519***	(0.0090)	-0.593***	(0.0050)
Union member	0.400***	(0.0106)	0.317***	(0.0059)
Health very good	-1.665***	(0.0088)	-1.664***	(0.0050)
Health very satisfactory	1.474***	(0.0122)	1.361***	(0.0069)
Prefers to work more	0.472***	(0.0166)	0.756***	(0.0091)
Prefers to work less	0.723***	(0.0088)	0.870***	(0.0050)
Income	-0.271***	(0.0099)	-0.137***	(0.0056)
Inverse Mills Ratio	0.102***	(0.0260)	-0.549***	(0.0240)
Year dummies	Yes		Yes	
Industry dummies	Yes		Yes	
Region dummies	Yes		Yes	
Constant	20.378***	(0.0782)	20.458***	(0.0461)
# of observations	16,972		51,532	
R^2	0.0851		0.0812	

Notes: This table presents the results of the second step OLS regression of the general happiness score on a set of observed covariates (excluding the interviewer dummy) and the inverse Mills ratio calculated from the results of the first step probit regression, which are given in Table (3.5). Clearly, the inverse Mills ratios indicate a significant positive-selection into the treatment sector. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Appropriate sampling weights are used. Robust standard errors are reported.

Table 3.8: Estimated treatment effects

Treatment Effects		
	Job Satisfaction	Happiness
Aggregate		
ATE	0.033	0.094
TT	0.211	0.936
TUT	-0.007	-0.179

Notes: This table documents the treatment effects estimates for job satisfaction and happiness. ATE refers to the Average Treatment Effect, TT refers to the (average) Treatment on the Treated, and TUT refers to the (average) Treatment on the Untreated. Appropriate sampling weights are used.

Table 3.9: Estimated treatment effects for education categories

Treatment Effects for Education Categories		
	Job Satisfaction	Happiness
Higher degree		
ATE	0.143	0.660
TT	0.311	1.473
TUT	0.098	0.369
First degree		
ATE	0.068	0.124
TT	0.240	0.958
TUT	0.025	-0.153
A-levels		
ATE	0.069	0.117
TT	0.247	0.972
TUT	0.029	-0.148
O-levels		
ATE	0.028	0.169
TT	0.205	1.110
TUT	-0.013	-0.104
Other higher qual.		
ATE	0.012	-0.013
TT	0.190	0.830
TUT	-0.028	-0.285
Vocational qual.		
ATE	0.014	-0.063
TT	0.186	0.781
TUT	-0.023	-0.334
No qual.		
ATE	0.014	0.146
TT	0.198	0.991
TUT	-0.023	-0.125

Notes: This table documents the treatment effect estimates for job satisfaction and happiness in different education categories. Appropriate sampling weights are used.

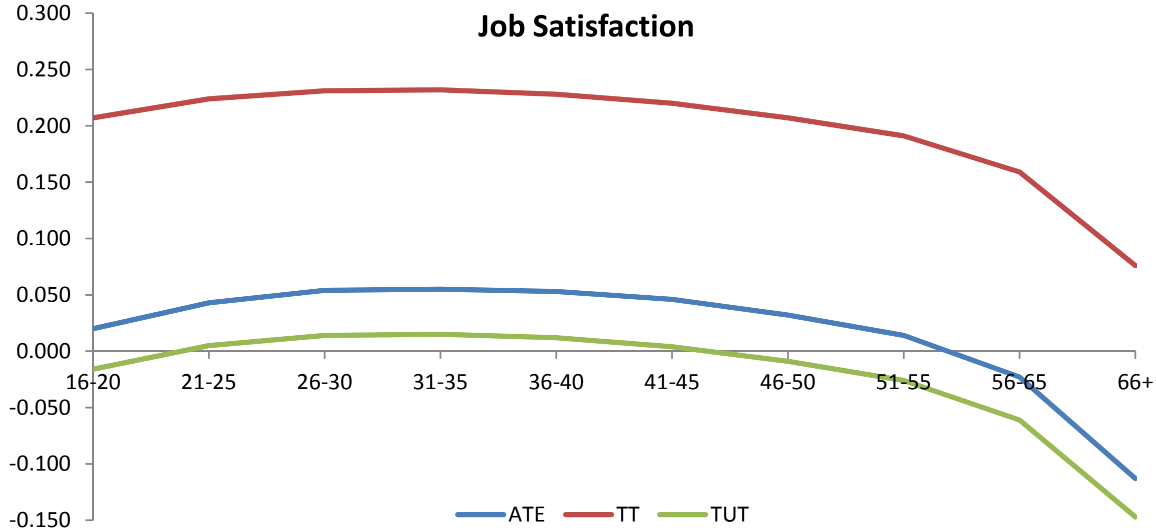
Table 3.10: Estimated treatment effects for sub-groups

Treatment Effects for sub-groups

	Job Satisfaction	Happiness	Job Satisfaction	Happiness
	Male		Female	
ATE	0.054	0.055	0.012	0.132
TT	0.230	0.894	0.191	0.978
TUT	0.013	-0.220	-0.027	-0.138
	Married		Non-married	
ATE	0.015	0.080	0.058	0.110
TT	0.192	0.925	0.236	0.950
TUT	-0.025	-0.190	0.018	-0.164
	Permanent		Temporary	
ATE	0.035	0.101	-0.073	-0.150
TT	0.212	0.942	0.101	0.715
TUT	-0.005	-0.172	-0.115	-0.409
	Public sector		Private sector	
ATE	0.067	0.326	0.026	0.046
TT	0.244	1.188	0.203	0.884
TUT	0.026	0.065	-0.014	-0.229
	Small employer		Large employer	
ATE	0.016	0.124	0.071	0.026
TT	0.194	0.965	0.247	0.871
TUT	-0.024	-0.149	0.030	-0.244
	Union worker		Non-union worker	
ATE	0.073	0.206	0.020	0.058
TT	0.250	1.056	0.197	0.899
TUT	0.033	-0.062	-0.020	-0.215
	Health very good		Health satisfactory	
ATE	0.059	0.063	0.033	0.162
TT	0.236	0.900	0.212	0.994
TUT	0.018	-0.212	-0.007	-0.117
	Prefers to work more		Prefers to work less	
ATE	-0.031	-0.087	0.060	0.005
TT	0.150	0.756	0.237	0.844
TUT	-0.069	-0.359	0.019	-0.269
	Higher relative income		Lower relative income	
ATE	0.052	0.058	0.010	0.132
TT	0.226	0.897	0.191	0.979
TUT	0.010	-0.216	-0.029	-0.137

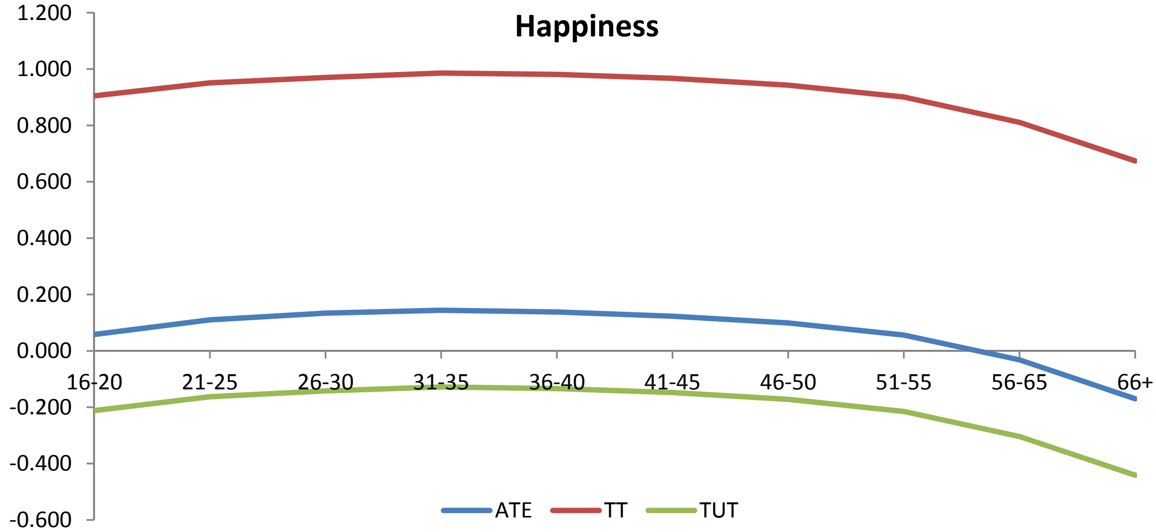
Notes: This table documents the treatment effect estimates for job satisfaction and happiness in certain sub-groups determined based on worker- and job-related characteristics. Appropriate sampling weights are used.

Figure 3.3: Treatment effects by age categories (job satisfaction).



Notes: This figure presents the estimated ATE, TT, and TUT categories for age groups in the job satisfaction analysis. Ten age categories are used. Appropriate sampling weights are used.

Figure 3.4: Treatment effects by age categories (happiness).



Notes: This figure presents the estimated ATE, TT, and TUT categories for age groups in the happiness analysis. Ten age categories are used. Appropriate sampling weights are used.

CHAPTER 4

ELECTIONS AND SUBJECTIVE WELL-BEING IN SUB-SAHARAN AFRICA

4.1 Introduction

Ethnic identity is an important determinant of people's lives in Sub-Saharan Africa. It affects who they trust, conduct business with, and vote for. Moreover, it can influence individuals regarding their overall well-being. It is often debated in the literature whether the importance of ethnic identity is driven by social or political affairs. Some scholars argue that ethnic identification comes from culture; that is, how people have lived throughout the centuries. Others argue that it is a political construct; political parties in many African countries use ethnic identities as a tool to gain access to political power. Using the Afrobarometer[1], Eifert, Miguel, and Posner (2010) show that ethnic identification is more prominent during election periods in comparison to other identifying categories such as gender, religion, and class/occupation. Ethnic attachments become even stronger if elections are in a competitive environment. More specifically, they show that respondents are 1.8 percentage points more likely to identify ethnically for every month closer the country is to a

[1]Afrobarometer surveys are conducted in 35 African countries and are repeated on a regular cycle. It measures social, political, and economic atmosphere of countries. See Section-4.2 for more information.

competitive presidential election. This suggests that ethnic identities in Africa are strengthened by political competition.

Competitive elections can cause considerable violence and widespread destruction of property, most of which is ethnically motivated. In ethnically diverse countries, political parties have used ethnic identity to mobilize voters and to establish political alliances, leading in some cases to violent ethnic conflicts. For instance, in Kenya, which is an ethnically diverse society, the 2007's presidential election resulted in the loss of 1,200 lives and the displacement of over a quarter of a million people. That election was one of the most competitive in the history of the country. This raises the questions about how competitive elections affect individual subjective well-being[2] when they are proximate and since competitive elections increase the salience of ethnic identification, how ethnic identification is related to subjective well-being after controlling for electoral cycle variables.

The first main interest of the paper is to investigate whether individual subjective well-being decreases when competitive elections are approaching. Due to the intense environment of competitive elections, the general tendency is to expect to observe a fall in the individual well-being in time the survey is to an election and further that this decrease is greater in a competitive election than a landslide election, which leads us to our first hypothesis. As an exercise under this hypothesis, we elaborate underlying mechanisms of the relationship that we will explain further in more detail.

The second main interest explored in the paper is the issue of subjective well-being of the individual and its relationship with the ethnic identification. Since the salience of ethnic identification is from political competition, this leads to expect that the higher the identification is, the lower is the individual well-

[2]Throughout the text, "happiness" and "subjective well-being" are used interchangeably and refer to an evaluation of one's own life considered as a whole.

being, stated as our second hypothesis. In addition, as the literature indicates, ethnic diversification deteriorates income distribution and creates poverty. In this respect, communities that are higher in diversification have lower levels of individual well-being.

These expectations are worth to test because the challenge facing Sub-Saharan African countries is how to keep the momentum of reforms going and at the same time improve the well-being of the people in order to avert social and political instability. Moreover, recent literature suggests that happiness and life satisfaction are also positively correlated with productivity that will boost economic growth [Zelenski, Murphy, and Jenkins (2008), Oswald, Proto, and Sgroi (2014)]. In order to implement policies, one should also take into consideration the factors to which the individual well-being is related.

One factor that some political scientists and economists identify as a cause of instability and poor economic growth is ethnicity. There is considerable literature documenting an inverse relationship between social heterogeneity and economic growth [Easterly and Levine (1997); Montalvo and Reynal-Querol (2005)].[3] Easterly and Levine (1997)'s famous "growth tragedy" is primarily based on the strong link between ethnic heterogeneity and slow growth in Sub-Saharan Africa (SSA).[4] However, this study has been criticized for employing ethnic fractionalization—known as ELF (ethno-linguistic fractionalization)—as a measure of ethnic diversity. The main criticism pertains the assumption upon which ELF is built [Posner (2004); Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003); Fearon (2003); Roeder (2004)]. However, there are several other studies that prove the negative link by using different in-

[3] A high level of ethnic diversification tends to affect economic growth and development processes such as trust and transaction costs [Leigh (2006); Knack and Keefer (1997)], provision of public good [Kimenyi (2006); Fosu, Bates, and Hoeffler (2006)], contact and contracts [Bates (2000)], and the level of investment [Mauro (1995)].

[4] They have documented that moving from an ethnically homogeneous country to one with a diversity of ethnic communities corresponds to a decrease in annual economic growth rates of more than two percent. These findings have been applied to Africa due to the high ethnic diversification of these countries.

dexes. Montalvo and Reynal-Querol (2005) utilize the index of polarization and conclude that polarization increase the probability of conflict. Esteban and Ray (2011) show that ethnic conflict is a linear combination of ethnic polarization, ethnic fractionalization, and the Greenberg-Gini index of inter-group differences. The weights of the linear combination correspond to the relative importance of public and private goods in the conflict. Specifically, the impact of polarization increases with conflict over public goods, while the impact of fractionalization increases with the private component of conflict. In their following paper, Esteban, Mayoral, and Ray (2012) show that a measure of polarization constructed using linguistic distances is a robust predictor of conflict. In this paper, we analyze the ethnicity with a different approach; primarily we try to estimate the relationship between well-being and the electoral cycle factors —where ethnic identification is more salient— and then the relationship between well-being and ethnicity.

We test these hypotheses with several well-being questions, provided by the Afrobarometer across 12 African countries. We find strong and robust evidence that political competition increases individual-level subjective well-being. The change in well-being is related to how close in time the survey is to an election and this proximity effect depends on the competitiveness of the election. Subjective well-being increases more in a competitive election period, in which the margin of victory is near zero compared to a landslide election. For every month closer a country is to a competitive election, on an average the individual-level subjective well-being demonstrates a 0.015 standard deviation increase. Since ethnic attachments grow stronger with political competition, we would expect to observe a positive relationship between ethnic attachment and subjective well-being. This is exactly the pattern: individuals who identify themselves ethnically have a higher subjective well-being than those who identify categories such as religion, gender, and class/occupation.

There are several possible mechanisms that account for these relationships. The first step in investigating this is to test the effects of winning elections on individual subjective well-being. Pierce, Rogers, and Snyder (2013) examine the immediate hedonic impact of electoral loss and victory to well-being. They conclude that elections strongly affect the well-being of partisan losers (for about a week), but minimally impact partisan winners. Moreover, Kahneman, Diener, and Schwarz (1999) suggest that partisan identity has considerable implications for the growing literature on well-being in economics, psychology, and other fields. The results show that winning the national election increases subjective well-being. A second possibility may be that individual subjective well-being increases as election day approaches, but then starts to fall gradually. We find that the proximity —before and after the election— is positively related to subjective well-being, but the impact before the election is greater than that after the election. The third one may be focused on whether there is any link between public expenditure and individual well-being. We find that the public expenditure on defense increases the individual-level subjective well-being. A fourth possibility may be that having participated in politics can increase subjective well-being. Stutzer and Frey (2006) show that in Switzerland engaging directly in the democratic process through referenda increases life satisfaction. Discussing politics and interested in public affairs have a positive impact on subjective well-being in SSA. The fifth and last mechanism that voting in free and fair elections improves the well-being of the individual. In addition of these mechanisms, ethnic identification might be seen as a group to which one wishes to belong. At election time politicians who play the ethnic card strategy might be increasing individual well-being via group/team effect.[5]

This paper also documents how individual-level variables are related to sub-

[5]The scope of this paper is to understand the effects of elections, and is a call for further research.

jective well-being. Overall, being older, employed, having higher education, living in urban areas, and having higher income have a positive impact on subjective well-being. Education especially has an enormous influence on the happiness level of the individual.

The econometric framework employs the same model as that of Eifert et al. (2010), which tries to capture the effects of proximity, the competitiveness of national elections, and the interaction of both variables, while controlling for large sets of individual variables such as gender, age, age-squared, rural/urban areas, education levels, employment status, and economic conditions. The Afrobarometer enables employing country fixed effects that control for country-level features, including unobservable characteristics that can not be measured since they have been collected not only across multiple countries but also at multiple points in time for the same countries. This creates a major advantage in testing the election variables that vary within countries across survey rounds.

The literature on subjective well-being and elections is very limited. So far there are two methods to analyze the relationship between elections and subjective well-being. One is to conduct a survey two days before and two days after elections [Pierce, Rogers, and Snyder (2013)], which is a very difficult dataset to construct. The other method is to use a panel dataset [Powdthavee, Dolan, and Metcalfe (2008)]. They test whether subjective well-being affects voting intentions, and the result of the election affects subjective well-being by using the British Household Panel Data (BHPS).[6] They find evidence that subjective well-being can affect voting intention but no evidence indicates that the results of three recent elections have had any effect on subjective well-being in the United Kingdom. They make use of the general elections in the UK in May 1997, June 2001, and May 2005. The BHPS takes place between September and December every year. Therefore, the wave before an election is six to nine

[6] The BHPS provides information on individual, household, and job/employer-related characteristics from 1991 to 2008 in England, Scotland, Wales, and Northern Ireland.

months, and the wave after the election is roughly three to six months. The Afrobarometer is a cross-sectional dataset and the easiest way to capture the impact of elections on the subjective well-being is to create a variable such as "electoral proximity". Since it is a cross-section dataset, we cannot follow the individuals: however, there is something of an indication as to the average impact every month closer to the election on individual-level subjective well being.

Competitive elections and ethnic identification produce a higher level of individual well-being. These results should be taken into consideration when implementing policies opposed to politically induced ethnic identification. Ethnicity can help to develop society, both socially and economically, by mobilizing people to initiate development projects in their communities. Glennerster et al. (2013) show that ethnically diverse communities have levels of collective action that are statistically indistinguishable from homogeneous communities in post-war Sierra Leone, one of the worlds poorest and most ethnically diverse countries. Moreover, Habyarimana et al. (2007) explore the impact of ethnic diversification in a laboratory environment. Their policy oriented conclusion is that generating higher levels of public good in heterogeneous communities does not require the segregation of ethnic groups. The challenge is to generate effective cooperation in diverse societies. Institutions are important for conducting effective policies to overcome high level of ethnic identification.

The plan of the paper is as follows. Section 4.2 provides an overview of the dataset, justifies the construction of dependent and independent variables, and explains the details of the econometric model. Section 4.3 presents the estimates, and discusses in detail the results summarized above. Section 4.4 concludes.

4.2 Data and Methodology

4.2.1 Data and Summary Statistics

The paper utilizes the Afrobarometer from Round 1 to Round 4 - the latest survey round available. The Afrobarometer measures the social, political, and economic atmosphere in Africa at an individual-level with a cross-sectional approach. The survey collects detailed information about the respondents' individual characteristics, views about democracy, governance, livelihoods, economic concerns, social capital, conflict and crime, their participation in the electoral process, and perceptions about national identities. Each survey employs the same sampling methodology and includes a large, nationally representative sample of individuals.

Twelve Sub-Saharan African countries are used in this study[7]: Botswana, Ghana, Lesotho, Malawi, Mali, Namibia, Nigeria, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe.[8] The survey that we employ spans from 1999 and 2009 covering almost ten years of information from each country. In order to account for country fixed effects, all countries exist in each survey round. To achieve national representativeness, appropriate weights and clustered samplings have been used. Weights are calculated as 1/(number of observations for that country).

The Afrobarometer includes several candidates for well-being:

1. *Your Present Living Conditions* - "In general, how would you describe your own present living conditions?"

2. *Your Living Conditions in 12 Months* - "Looking ahead, do you expect the following to be better or worse?: Your living conditions in twelve

[7]Number of observations in the regressions varies depending on the availability of the variable in rounds.

[8]See Table (4.1) for detailed information of countries and survey years.

months time?”

3. *Your Living Conditions Compared to 12 months Ago* - “Looking back, how do you rate the following compared to twelve months ago?: Your living conditions?”

4. *Mental Health* - “In the last month, how much of the time: Have you been so worried or anxious that you have felt tired, worn out, or exhausted?”

5. *Your Living Conditions Compared to Others* - “In general, how do you rate your living conditions compared to those of other in your country?”

6. *Ethnic Group Economic Conditions* - “Think about the condition of your ethnic group. Are their economic conditions worse, the same as, or better than other groups in this country?”

The first three questions' responses are based on a five point scale with 1 representing “very bad”, 2 “fairly bad”, 3 “neither good nor bad”, 4 “fairly good” and 5 “very good”. “Mental health” question is also reported based on a five point scale ranging from 1 (never) to 5 (always). “Your living conditions compared to others” and “ethnic group economic conditions” are ranked on a five point scale on which 1 indicates “much worse” and 5 indicates “much better”.

To streamline the interpretation and draw a general conclusion, we analyze hypotheses with three different dependent variables to create indices. We create summary indices aggregating information across related outcomes of similar subjective well-being questions [Kling, Liebman, and Katz (2007)]. The main motivation for this grouping is to improve the statistical ability to detect effects that are consistent across specific outcomes when these specific outcomes also have idiosyncratic shocks. Following the methodology of Kling, Liebman, and Katz (2007), we create summary indices based on specific outcomes, in which specific outcomes are normalized by subtracting the mean of the group and then dividing by the standard deviation of the group. Formally, X_i is the i-th of I outcomes; let μ_i be the group mean and let σ_i be the standard deviation of the group. The normalized outcome is $X_i^*=(X_i-\mu_i)/\sigma_i$. The summary index is $X^*=\sum_i X_i^*/I$. Overall, the summary index is defined as the weighted average of z-score of its components. The z-scores are normalized scores based on the group mean and standard deviation. As stated in the Table (4.1), each component of the index has a mean of zero and a standard deviation of one.

The first outcome is for "your living conditions" which is a combination of the first three questions. These questions respectively evaluate the individual's current situation, future condition and the comparison of current and past living conditions, in an attempt to measure well-being in a time perspective. In this respect, they are from the same domain, which enables aggregate information across related outcomes. The mean of "your living conditions in 12 months" is higher than the other two living condition variables, most probably due to the individual's high expectations and aspirations for the future. The mean of comparison of current and past living conditions using "your living conditions compared to 12 months ago" is higher than the current living conditions, which might roughly be interpreted as an indication that the people are becoming happier. The second outcome is the combination of "mental health" with a mean of 3 and a standard deviation of 0.95. We reversed the signs for mental health, so that higher values correspond to higher subjective well-being for all outcomes. "Mental health" question can be evaluated as the General Health Questionnaire (GHQ) since the GHQ also measures whether a respondent suffers from a health problem related to anxiety or depression. The last outcome is for "your conditions compared to others", which is a combination of the last two questions. This assumes a reference group of language/tribe/ethnic group. This assumption becomes stronger with the similar mean approximately 2.8.[9]

[9] All regressions were also tested separately using the six well-being questions on the

For the individual characteristics we control for gender, age, age-squared, rural/urban areas, education levels, employment status, and economic conditions. The variables of economic conditions are the indices of the following questions, “Over the past year, how often, if ever, have you or anyone in your family gone without enough food to eat, enough clean water for home use, medicines or medical treatment, and a cash income?”. To test the relationship between subjective well-being and ethnic identification, we make use of the personal identification question; “Besides being [a citizen of X], which specific group do you feel you belong to first and foremost?” We group responses into five categories: language/ethnic group/tribe, religion, occupation/class, gender, and other. The other category stands for race, region, age, “I'm my own person”. We adopt country-fixed effect framework, which automatically controls for many other aspects of country: level of economic development, history, civil war, etc.[10] All regressions include round dummies and are clustered by countries.

Nearly half of the individuals in the sample are male with an average age of 37. We restrict the sample minimum age to 18, which means that all individuals have the right to vote. Approximately 37% of individuals in the sample live in urban areas. We collapse education into seven categories. Post-graduate refers to graduate studies with 0.4% of the sample meeting this criterion. Nearly 3% graduated from a university. The highest share in education falls into primary and secondary school. Less than half of the sample is unemployed. As a proxy of income variable we control for economic conditions, which is an index that averages together income related variables such as how often the respondent had gone without food, water, medical care, and income. Nearly 35% of individuals in the dataset identified themselves with the occupation/class, 26% indicated that they belonged to a language/ethnic/tribe, 16% stated religion

list. The results (not shown) are parallel with the indices results. They are available upon request.

[10] Results are robust even if dropping a single country in the dataset.

and identifying themselves in their gender group identification has the smallest number of responses at 0.4%.

Table (4.2) summarizes economic and political characteristics of sample countries. The average GDP per capita of sample countries is higher than the average of Sub-Saharan Africa, which is mainly driven by Botswana, Namibia, and South Africa. The other countries are poorer on an average in the SSA. Rates of urbanization are almost the same level as the SSA average. Utilizing at least two[11] round surveys brings variations in the months to election variable, which is called "proximity to the election" in the regression. In Botswana, for example, -1 means that the survey round occurred one month before the election and 15 means the survey round occurred 15 months after the election. The competition of presidential election is measured by vote margin, and is simply the vote share difference between the winner and the runner-up. The competitiveness level in sample countries is similar to the African average. Utilizing three rounds of the Afrobarometer brings more variation in the competitiveness variable since we can make use of more elections. The last column stands for the name of the ruling party during that election period. This variable is utilized for examining whether winning a competitive election changes individual-level subjective well-being.

4.2.2 Empirical Methodology

The econometric model is designed to illuminate the influence of proximity of election, competitiveness of election, and interaction of these two effects to subjective well-being. In the model i represents the individual respondent, c is for country, and t denotes the survey round as attached to individual subjective well-being SWB_{ict}. Within this setup, we can systematically analyze the extent to which individual-level subjective well-being is related to observable

[11]It can be three depending on the dependent variable.

characteristics and a country's political environment.

$$\mathbf{SWB}_{ict} = \beta_0 + \beta_1\mathbf{X}_{ict} + \beta_2\mathbf{C}_{ct} + \beta_3\mathbf{p}_{ct} + \beta_4\mathbf{c}_{ct} + \beta_5(\mathbf{p}_{ct} * \mathbf{c}_{ct}) + \epsilon_{ict} \tag{4.1}$$

The vector $\mathbf{X}_{ict}$ represents individual-level variables[12], $\mathbf{C}_{ct}$ for country-level factors; and ϵ_{ict} is individual's idiosyncratic level. The focus of the paper is on election variables, $\mathbf{p}_{ct}$ is a proximity variable that measures months until the election in the country compared to the survey round. In Table (4.2), negative numbers indicate the most recent past election. Proximity is coded as -1*abs(months to/from the most recent election) so that larger numbers imply increasing proximity. $\mathbf{c}_{ct}$ is a competitiveness variable and defined as vote margin, which is the gap between the vote share of the winner and the runner-up in the most recent election. The competitiveness variable is calculated from vote margin as -1*(vote margin). Larger numbers indicate increasing competitiveness. $\mathbf{p}_{ct} * \mathbf{c}_{ct}$ is the interaction variable of proximity and competitiveness.

The estimates we report and discuss in Section 4.3 refer directly to "marginal effects". Formally,

$$\frac{\partial SWB_{ict}}{\partial p_{ct}} = \beta_3 + \beta_5 c_{ct} \tag{4.2}$$

$$\frac{\partial SWB_{ict}}{\partial c_{ct}} = \beta_4 + \beta_5 p_{ct} \tag{4.3}$$

Since the dependent variable is ordinal rather than cardinal, the ideal way to carry out analyses is through ordered probit. However, Ferrer-i-Carbonell and Frijters (2004) demonstrate that the results from cardinal analysis using OLS is very similar to those from ordinal analysis. For ease of interpretation, the

[12] See Table (4.1) for individual-level variables.

equation is estimated by using OLS.[13]

4.3 Results and Discussion

This section documents the empirical results and provides an extensive discussion of the impact of election variables on subjective well-being and the possible underlying mechanism of this relationship. We also stress the relationship between personal identification and subjective well-being and the influence of individual characteristics on subjective well-being. Moreover, we conduct the same exercise of Eifert et al. (2010) with this dataset to show the salience of ethnicity when competitive elections are near.

We estimate Equation (4.1) using the Afrobarometer controlled for individual characteristics, country, and round dummies on different well-being measures. Note that "marginal effects" are reported, which means that estimates are readily interpretable in terms of our parameters of interest.

4.3.1 Effects of Proximity to Competitive Elections

This section provides a discussion of electoral cycle variables. All three subjective well-being measures generate almost identical results: the impact of elections on subjective well-being is positive and significant. This leads us to reject the first hypothesis. The individual well-being increases by 0.015 standard deviation when competitive elections are approaching.

Figure (4.1) shows the proximity to the closest country election on the x-axis and the predicted subjective well-being (your living conditions) on the y-axis by competitiveness of national elections.[14] Results are documented in two

[13]The results of the ordered probit model (not shown) are in line with the OLS model. After controlling for individual characteristics, country, and time dummies "your living conditions" increases by 0.0173 standard deviation, "your living conditions compared to others" by 0.014 standard deviation, "mental health" raises by 0.012 standard deviation when competitive elections are near. The results are available upon request.

[14]Since there is a robust result among dependent variables, we conduct this analysis with

groups; high competitiveness, in which the median of the electoral margin is less than the sample median of 36 percentage points and less competitiveness (landslide elections), in which the electoral margin is more than the median sample. The relationship is clearly evident. Landslide elections —even when in the proximity of elections— yield a lower subjective well-being compared to relatively higher competitive elections. Subjective well-being becomes higher as time nears the elections. In other words, elections have a positive impact on subjective well-being of individuals in Sub-Saharan Africa but only where elections are meaningful contests for political power.

Tables (4.3.1), (4.3.2) and (4.3.3) report the tests of the first hypothesis under three specifications. All specifications include country fixed effects, weigh each observation by 1/(number of observations from that country) to weigh each country survey round equally and include standard errors clustered at the country level.

The first columns in Tables (4.3.1), (4.3.2) and (4.3.3) suggest mixed results. In the case of "your living conditions" the proximity of the survey to a presidential election (in months, absolute value) on average decreases the living conditions of the individual. However, the competitiveness of that election (the margin of victory) has a positive impact on subjective well-being. "Your living conditions compared to others" has exactly opposite results; the competitiveness of the election decreases the happiness level, while happiness is increased as the election draws nearer. When competitive elections are held sooner, the mental health level of the individual decreases. However, the difference in the "electoral competitiveness" variable is minimal, especially in "mental health", which is present only in two rounds of the survey. Specifically, six out of twelve countries experience a different election. The dataset allows analysis of "your living conditions" in three rounds, so in that case the variation becomes higher

only the "your living conditions" variable for the figure.

when all countries have experienced at least one different election. Given these concerns, it is wise to pay attention to interaction terms between proximity and competitiveness. After adding the interaction term to the regression (the second column of Tables ((4.3.1), (4.3.2) and (4.3.3)), positive and statistically significant coefficients of election variables among all dependent variables are obtained. More specifically, every month closer a country gets to a competitive election, on average individual-level subjective well-being increases 0.011-0.017 in standard deviation depending on the dependent variable of well-being. Moreover, the higher the competitiveness of the election, the greater the subjective well-being of the individual. The results are confirmed in Column 3, which controls for individual-level characteristics such as age, gender, education, economic conditions, and urban or rural residence. Among all dependent variables, the results are quite consistent with each other, and they are identical among the last two specifications.

Our first hypothesis was expected to a fall in the individual well-being due to the intense environment of competitive elections. However, the results suggest the opposite. In the following sections, we test what could be the underlying mechanism of this results. Potentially, there could be three leading reasons: (a) People would like to be in an union, where they support the same ideology and fight for it; in our context union might be "ethnic identification"., (b) People would like to feel happy if they observe their supported party is close to win the elections., (c) Or simply participating politics in any way might give a feeling of contributing country's political and economical affairs.

4.3.2 Effects of Ethnic Identification

Table (4.4) documents the importance of personal identification on well-being. The regressions account for individual characteristics, country and time dummies, and election variables.[15] As mentioned above, the personal identification question is derived from the specific question, which is available only in Rounds 1 and 2. Given the availability of the dependent variable, it is possible to run the regression only for Round 2. We also reject the second hypothesis. Having a personal identification as your ethnic group or religion has a positive and statistically significant impact on subjective well-being of the individual, controlling for electoral cycle variables. Gender also plays a role in that purpose but this result should be evaluated with caution since a low percentage of individuals identified themselves with their gender.

Identifying oneself in an ethnic group may be linked to belonging to a group/team, which in turn increases happiness levels. When we conduct the analysis without electoral cycle variables, the coefficient of ethnic identification is smaller compared to controlling for electoral cycle variables. This suggests that the more pronounced the ethnic identification, the higher is the subjective well-being of the individual. Unlike general expectations about high ethnic diversification creating lower individual well-being, individuals who identify themselves as their ethnic group report higher well-being. Social heterogeneity in Sub-Saharan Africa can make use of this information for policy. It may be the case that politicians who play the ethnic card strategy in their election campaign stimulate the well-being of individuals.

When the individual evaluates their living conditions based on a time preferences, which is "your living conditions", this is negatively related to ethnic identification. However, the other two dependent variable are in parallel with each other and positively related. There might be several mechanism behind this relationship. Several studies are argued that there are cultural differences between the ethnic groups regarding subjective well-being. Ethnic groups have different conceptions of well-being and that different factors influence their sub-

[15] The same regressions have been run without election variables (not shown), which is in line with the previous one. The results are available upon request.

jective well-being [Neff (2007), Agyemang et al. (2013)]. For instance, Addai and Pokimica (2010) report that ethnicity is an important determinant of perceived economic well-being of individuals in Ghana. In their analysis ethnicity tends to have both negative and positive effect on economic well-being among different ethnic groups and different sub-sample.

Religion identification yields positive relationship with all dependent variables and has statistically significant in “your living conditions” and “your living conditions compared to others”. It is a well-known fact that individuals with strong religious beliefs report a higher level of life satisfaction and greater personal happiness [Ellison (1991), Ellison, Boardman, Williams, and Jackson (2001)]. Religion is also shaped perceived economic well-being of individuals in Ghana [Pokimica, Addai, and Takyi (2012)].

4.3.3 The Salience of Ethnicity

This subsection conducts the same exercise of Eifert et al. (2010) with our dataset. The dataset differs from their dataset in terms of sample countries —Ghana and Lesotho are included— and some individual characteristics such as the economic conditions. The main motivation is to observe whether the ethnic identification is more pronounced during competitive elections with our dataset. We run regressions using a multinomial logit model and ordinary least square and control for individual characteristics and time and country dummies. The results in Table (4.5) are in line with Eifert et al. (2010). Every month closer to competitive elections, survey respondents are on average 1.8 percentage points more likely to identify themselves in ethnic terms.

Since 1990, the banning of ethnic parties has become the norm in Sub-Saharan Africa. In our dataset the ethnic banning countries are Tanzania and Uganda. For instance, Tanzania has used the education system and redistribution of resources to develop a sense of national as opposed to ethnic identity. The studies that show the impact of ethnic banning in parties conclude that these laws have only marginally influenced the character of the political parties [Moroff (2010)]. Ethnic banning may alter the origin of parties, resulting in ethnic-free parties. This affects voting behavior and subjective well-being. Ethnic banning can also influence the salience of ethnic identification. Tanzania has among the lowest degree of ethnic identity salience in one of the Afrobarometer survey rounds, at just 3%. Eifert et al. (2010) also show Tanzania's outlier status. The presidential election has little impact on the share of the population that identifies themselves in ethnic terms. In Figure (4.1), one can also observe that the impact of the proximity of elections on subjective well-being is less in Tanzania compared to other countries. Tanzania's situation is proof of the strength of ethnic identification in politics.[16] Miguel (2004) examines the success of nation-building policies in Tanzania, which have had a beneficial long-run impact on country's political stability and economic development.

4.3.4 Background Mechanisms of Elections and Subjective Well-Being

It has been clearly documented that elections make people happier. This finding requires more research to understand the underlying mechanism of the positive relationship. There might be several channels but the most pronounced ones are winning elections, ex-ante and ex-post impact of elections, the effects of public expenditure such as education, health, and defense, the effects of having actively participated in politics, and trust in the national electoral commission. This part of the paper explains these leading background relationships.

[16] However, subtracting these countries in the regression of ethnic salience and the impact of elections to the subjective well-being does not alter the results. We cannot conduct analyses based in Tanzania and Uganda due to high collinearity of election variables.

Effects of Winning Elections. The model is tested to determine whether winning the election has an influence on subjective well-being. The dummy variable is created for that purpose utilizing the following question "Do you feel close to any particular political party or political organization? If so, which party or organization is that?" Since the winning party of this election is known, if the winning party is the same as the answer from the individual, it is scored as a one; otherwise it is scored as zero. Using the same model the results show that winning the competitive election increases individual-level subjective well-being. "Your living conditions" and "your living conditions compared to others" yield positive and statistically significant results. Winning the election is positively related with "Mental health" dependent variable but it is not statistically significant.

Individuals might think that winning the election is an economic privilege, employment opportunities, protection from possible threats in the future such as civil war, ethnic clashes etc., and easy access to health and education services. These opportunities increase the expectations and aspirations of individuals and lead to higher individual well-being. The joy of wining also increases well-being. Moreover, almost all countries in the dataset except for Malawi and Mali have had the same incumbent for at least three presidential elections. Winners might have perceived retaining the presidency as maintaining the status quo of ongoing policies, thereby raising happiness.

Asymmetrical Effects of Elections. Individual's expectations and aspirations may be higher before rather than after the election and these may form one's level of happiness. There is a strong possibility that subjective well-being increases as election day approaches but then starts to fall post-election. The model is designed for testing the symmetric effects of elections. In order to observe asymmetrical effects especially before the election, we create a dummy variable for countries in which round surveys would be completed prior to the nearest election. If the election to month variable is above (below) zero, it is referred to as "after (before) election" and carries a value of 1. The ex-ante and ex-post effects have a positive and statistically significant relationship with individual well-being.[17] The impact of the electoral cycle on subjective well-being is more powerful before the election than after the election.

During election campaigns the general tendency is to conduct populist policies such as expansionary fiscal policies —cut taxes, increase government spending, and subsidize small and medium sized enterprises— by the incumbent, providing food, water or other necessities that the people need, and gifts to entertain society. Block (2002) analyzes a number of fiscal and monetary variables in Sub-Saharan Africa during and after elections and concludes that governmental spending shifts toward more visible, current expenditures and away from public investment. This temporary help may increase individual well-being. In addition, individuals want to believe that something will change within their country with the coming of the election; this hope may yield a higher subjective well-being. The leading reason for observing positive well-being after an election may be due to a decrease in the intensity of the environment. Individuals can attain relief since the uncertainty deriving from the election is over.

Effects of Public Expenditure. There might be a potential link between public expenditure and voter's well-being. The individual may be happy during the period of the incumbent party due to an increase in public welfare. As mentioned in the previous paragraph, governments may try to influence their popularity around elections by increasing public expenditures. Thereby, we analyze whether a change in public expenditure on education, health, and defense can have an impact on the well-being of individual. We focus on change in public expenditures by sections such as education, health, and defense in-

[17] See Table (4.6.1) Only ex-ante effects of the electoral cycle are represented. The result of ex-post effects is available upon request.

stead of total change in public expenditure because politicians may change the composition of expenditure in an election year, without increasing the overall budget. We control for the change in public expenditure during the election year; when the election was held in Botswana in 1999, we account for the change in public expenditure from 1998 to 1999. We utilize the change in "government expenditure on education" for education and "government expenditure on health" for health, and defense is based on "government expenditure on military"[18].

Among three types of public expenditure, the impact of defense expenditure on individual well-being is the highest and statistically significant for satisfaction with your living conditions and satisfaction with your living conditions compared to others. The military spending is an important issue for SSA, which has been through considerable turmoil, with high levels of conflict in the region and within country, that especially increases during the election period. Individuals may feel secure when the government expenditure is higher on the military and this feeling of security may lead to increase their well-being. The relationship between health expenditure and individual well-being is only positive and statistically significant in satisfaction with your living conditions. Regarding education expenses, it is positively related with well-being however, is not statistically significant.

Effects of Participating in Politics. The Afrobarometer permits analyzing this mechanism in various ways. The main way is to look into whether discussing politics[19] and being interested in public affairs[20] increase individual well-being.

[18] Data is taken from World Development Indicators.

[19] The exact wording of discuss politics is "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Discussed politics with friends or neighbors?". It is a three point based scale, 0 refers to "No, would never do this" and 2 is "Yes, frequently". It is available only in Round 3 and 4.

[20] The formal question of interested in public affairs is "How interested are you in public affairs?". It is a four point based scale, 0 refers to "Not interested" and 3 refers to "Very interested". It is available in all rounds.

The mean of discussing politics is 0.91 and a standard deviation of 0.72. The mean of interested in public affairs is 1.78 and a standard deviation of 1.10. In line with each other, they are positively related with individual subjective well-being.[21] Thirdly, we check the impact of active participation such as attending a demonstration or protest march. The last and the fourth way may be that the interaction with political party official increases well-being of the individual. The individual can feel important if he/she discusses topics related with country someone in power. Both have no significant impact on any dependent variable. However, number of responses to the questions are low; more than fifty percent of the sample never attend a demonstration, and less than ten percent of the sample get in contact with a political party official.

Individuals in low-income countries participating in politics, after the passage of an election may feel particularly valuable. These individuals make a fundamental contribution to democratic governance in their country, which can change the future of the country. Moreover, when the electoral process is competitive and candidates or parties are forced to expose their records and future intentions to popular scrutiny, more discussion and interest in politics arises. Doing something valuable for one's country may produce a higher subjective well-being.

Effects of Illegitimacy of Elections. To gain a sense of the illegitimacy of elections, we control for "trust national electoral commission", which has a mean of 1.56 and a standard deviation of 1.1. The trust variable is positively and significantly correlated with subjective-well being. The higher the legitimacy in elections, the higher is the subjective well-being of the individual. Allowing people to freely choose from different alternatives in competitive elections increases political trust and those increases lead to greater subjective well-being.

[21] These variables are tested separately. Only the result of "interested in public affairs" is presented.

In Sub-Saharan Africa, vote buying and ballot fraud are serious problems during elections. In the Afrobarometer, the only variable to control for that purpose is "trust national electoral commission". However, this variable should be evaluated carefully since conditional correlations exist. An individual who thinks that elections are free and fair probably the one who wins the election: s/he already feels content about the election result. On the other hand, the loser starts blaming the commission since s/he is not happy with the result.

4.3.5 Effects of Individual-level Variables on Subjective Well-Being

Table (4.7) shows the relationship between individual-level subjective well-being and individual characteristics. Unlike findings in Western societies, being female is negatively correlated with mental health of the individual; however, females fare better when compared with other ethnic groups. Women in SSA are facing human rights abuses such as sexual discrimination and abuse, intimate violence, political marginalization, and economic deprivation. These may lead to have lower well-being. Older people are happier, which is in line with Western societies. Living in rural areas has a negative impact on well-being of individuals, and the coefficient becomes higher especially when respondents compare themselves to others. In SSA, those living in rural areas experience more poverty and less access to health care and education. Sahn and Stifel (2003) conduct a study in 24 African countries and conclude that standards of living in rural areas almost universally lag behind in urban areas.[22] Education has a huge, positive and significant impact on subjective well-being of the individual. It is a well-known fact that in Western societies highly educated individuals are less happy than high school, secondary, and primary school graduates, mostly because of higher expectations and aspirations from

[22]Education, school enrollments and the ratio of girl-to-boy enrollments is much lower in rural areas than in urban areas.

life, especially jobwise. However, in the case of SSA, education improves the well-being of individuals. There is room to gain from policies for education to increase the well-being of individuals. Not surprisingly, being employed is positively related to all dependent variables of well-being. If a person experiences economic difficulties, this decreases the happiness level of the individual and the coefficient is higher when they compare themselves to their ethnic group. It is clearly observed that an income-comparison[23] argument is also valid in that region.

4.4 Concluding Remarks and Discussion

As Eifert, Miguel, and Posner (2010) stated, the source of ethnic salience comes from political competition: in other words, proximity to competitive elections increases the strength of ethnic attachments. A general consensus exists about the negative relationship between economic development and social heterogeneity based on both cross-country regressions and individual country studies. This paper brings a different angle to the discussion of election, ethnic identification, and growth. It explores these phenomena under the umbrella of well-being by asking: "How do competitive elections affect individual-level subjective well-being?", and "Is there any relationship between ethnic identification and subjective well-being?" The results show that for every month closer a country is to a competitive election, on average individual-level subjective well-being has a 0.015 standard deviation increase. Moreover, if individuals identify themselves ethnically higher, this is positively correlated with individual-level subjective well-being. These findings are important for designing policies to increase social welfare in SSA.

These findings point to the background mechanism of this question: "Why do elections make people happier given that competitive elections in this region

[23] Income is evaluated relative to others (social comparison).

have a very intense environment?" We look into five possible mechanisms: winning the election, the effects of forthcoming election, the impact of public expenditure on education, health, and defense, the environment of free and fair elections, and participation in politics. These mechanisms have a positive impact on the well-being of individuals. The policy maker should internalize this positive externality of election and ethnic identification on individual-level well being. As stated in Habyarimana et al. (2007), enforcing cooperation among individuals, in this context the same ethnic group, would make policies more effective. Our results also suggest that individuals feel happier when they are identified themselves ethnically. Thereby, combination of these results might enable us to draw a conclusion such that policies may be implemented in an ethnic group-level rather than a country-level.

The findings of the paper should be treated very cautiously regarding policy implications. These positive well-being effects occur only when elections are proximate. In the short run, these positive externalities might boost economic growth, but the long-run implications are ambiguous. Moreover, there might be some events during competitive elections which could possibly alter the subjective well-being and these may create some bias in the results.

Apart from the empirical findings this paper has three good features for analyzing hypotheses. It creates indices of well-being questions to aggregate same outcomes across domains such as time and comparison. We have grouped three well-being questions that evaluate living conditions regarding time and two well-being questions based on comparisons of respondents' lives. A second methodological contribution is to make use of repeated country-level observations with micro-individual survey data. Since the data have been collected at multiple points in time for the same countries, it allows for variation in key parameters of interest such as the proximity of the survey to the nearest election and the competitiveness of that contest. Moreover, well-being of individuals is affected mostly by the characteristics of the social and political environment in which he or she lives. Using the feature of data, we employ a country fixed effect model to overcome country-level characteristics. Thirdly, these results are drawn from cross-national survey data rather than case studies and anecdotal evidence, which allows for generalized cross settings and creates a much stronger position.

Table 4.1: SUMMARY STATISTICS-AFROBAROMETER, ROUND 1-2-3-4

Variable	Mean	Std.Dev	Available Round
Dependent Variable			
1-Your Living Conditions	0	1	2-3-4
Your Present Living Conditions	2.61	1.22	2-3-4
Nor. Your Present Living Conditions	0	1	2-3-4
Your Living Conditions in 12 Months	3.39	1.25	1-2-3-4
Nor. Your Living Conditions in 12 Months	0	1	2-3-4
Your Living Conditions Compared to 12 Months Ago	3.01	1.131	1-2-3-4
Nor. Your Living Conditions Compared to 12 Months Ago	0	1	2-3-4
2- Nor. Mental Health	0	1	2-3
Mental Health	3.05	0.95	2-3
3-Your Conditions Compared to Others	0	1	3-4
Your Living Conditions Compared to Others	2.81	1.08	1-2-3-4
Nor. Your Living Conditions Compared to Others	0.	1	3-4
Ethnic Group Economic Conditions to Others	2.8	1.017	3-4
Nor. Ethnic Group Economic Conditions to Others	0	1	3-4
Individual Characteristics			
Male	0.50	0.5	1-2-3-4
Age	36.79	14.89	1-2-3-4
Urban	0.37	0.48	1-2-3-4
Post-Graduate	0.004	0.06	1-2-3-4
University	0.03	0.16	1-2-3-4
High School	0.07	0.25	1-2-3-4
Secondary School	0.37	0.48	1-2-3-4
Primary School	0.36	0.48	1-2-3-4
Informal Schooling	0.03	0.18	1-2-3-4
No schooling	0.13	0.34	1-2-3-4
Employed	0.37	0.48	1-2-3-4
Interested in public affairs	1,78	1,10	2-3-4
Trust national electoral commission	1.56	1.1	2-3-4
Economic Conditions	0	1	2-3-4
How often gone without food	3.02	1.07	1-2-3-4
Nor. how often gone without food	0	1	2-3-4
How often gone without water	3.08	1.11	1-2-3-4
Nor. how often gone without water	0	1	2-3-4
How often gone without medical care	2.95	1.1	1-2-3-4
Nor. how often gone without medical care	0	1	2-3-4
How often gone without cash income	2.58	1.17	1-2-3-4
Nor. how often gone without cash income	0	1	2-3-4
Personal Identification			
Occupation/Class	0.35	0.47	1-2
Language/Ethnic/Tribe Group	0.26	0.44	1-2
Religion	0.16	0.36	1-2
Gender	0.04	0.20	1-2
Other	0.17	0.38	1-2
# of observations		60,050	

Notes: Weights are calculated as 1/(number of observations of that country).
Stated number of observation is for independent variables in all rounds.
Number of observation for each dependent variable is noted in estimation results.

Table 4.2: Economic and Political Characteristics of Countries

	Economic Characteristics		Political Characteristics		
Country and Survey Year	GDP per capita($)	%Urban	Month to Election	Vote Margin	Ruling Party
Botswana,1999	7,727	52	-1	0.31	BDP
Botswana,2003	9,366	56	15	0.25	BDP
Botswana,2005	11,177	57	-8	0.25	BDP
Botswana,2008	14,104	60	12	0.31	BDP
Ghana,1999	1,390	43	12	0.04	NPP
Ghana,2002	1,560	45	22	0.08	NPP
Ghana,2005	2,030	57	-8	0.08	NPP
Ghana,2008	2,486	50	4	0	NDC
Lesotho,2000	1,019	19	-23	0.36	LCD
Lesotho,2003	1,172	22	-10	0.32	LCD
Lesotho,2005	1,330	23	19	0.28	NIP
Lesotho,2008	1,648	25	-20	0.28	NIP
Malawi,1999	556	14	-6	0.14	UDF
Malawi,2003	561	15	12	0.08	Coalition
Malawi,2005	605	15	-13	0.08	Coalition
Malawi,2008	727	15	6	0.36	DPP
Mali,2001	727	29	16	0.92	ADEMA
Mali,2002	747	29	-6	0.3	Coalition
Mali,2005	914	31	22	0.52	ADP
Mali,2008	998	33	-20	0.52	ADP
Namibia,1999	3,872	32	3	0.66	SWAPO
Namibia,2003	4,405	34	14	0.69	SWAPO
Namibia,2006	5,998	36	-15	0.69	SWAPO
Namibia,2008	6,596	37	13	0.64	SWAPO
Nigeria,2000	1,131	42	-10	0.26	Coalition
Nigeria,2003	1,597	44	-6	0.3	PDP
Nigeria,2005	1,795	46	18	0.51	PDP
Nigeria,2008	2,149	48	-13	0.51	PDP
S.Africa,2000	6,653	57	-13	0.57	ANC
S.Africa,2002	7,195	58	19	0.57	ANC
S.Africa,2006	9,319	60	-22	0.57	ANC
S.Africa,2008	10,250	61	6	0.49	ANC
Tanzania,2001	823	23	-7	0.53	CCM
Tanzania,2003	938	23	29	0.69	CCM
Tanzania,2005	1,073	24	4	0.69	CCM
Tanzania,2008	1,313	25	28	0.36	CCM
Uganda,2000	763	12	10	0.42	YKM
Uganda,2002	866	13	-17	0.42	NRM
Uganda,2005	1,014	13	10	0.22	NRM
Uganda,2008	1,268	14	30	0.42	NRM
Zambia,1999	899	35	26	0.04	MMD
Zambia,2003	1,055	36	-17	0.04	MMD
Zambia,2005	1,073	37	13	0.14	Coalition
Zambia,2009	1,367	38	-8	0.02	MMD
Zimbabwe,1999	885	33	7	0.02	ZANU-PF
Zimbabwe,2004	314	35	-26	0.14	ZANU-PF
Zimbabwe,2005	477	36	1	0.53	ZANU-PF
Zimbabwe,2009	425	38	-14	0.05	MDC
Avr, sample	2,840	35	14	0.35	*
Avr, SSA	1,606	37	*	0.34	*

Notes: Macroeconomic variables are taken from World Development Indicators. Political variables come from African Election Database.

Figure 4.1: Electoral Proximity and Subjective Well-Being, by Competitiveness of Elections

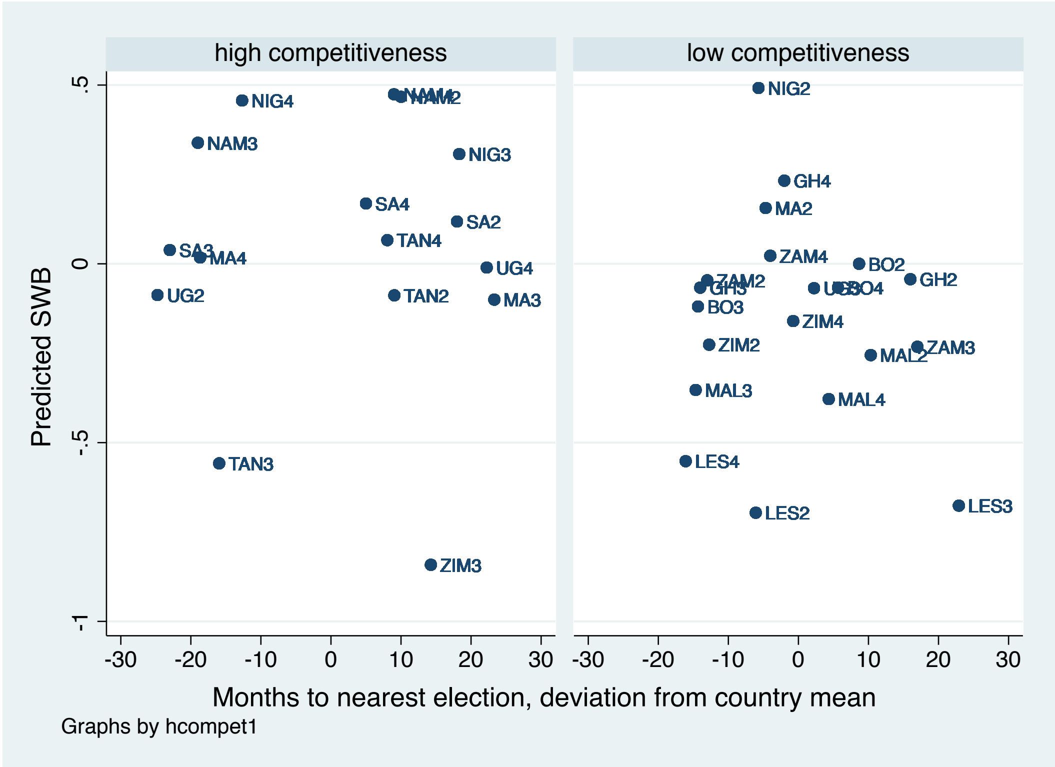


Table 4.3.1: Your Living Conditions and Electoral Cycle

Dependent Variable: Your Living Conditions

Electoral Proximity	-0.0048***	0.0173***	0.0172***
	(0.0007)	(0.0031)	(0.003)
Electoral Competitiveness	0.833***	1.64***	1.61***
	(0.0545)	(0.123)	(0.12)
Proximity*Competitiveness	-	0.0288***	0.027***
	-	(0.004)	(0.0038)
Individual Characteristics	No	No	Yes
Country Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Adj. R^2	0.1064	0.1078	0.1800
N	41,186	41,186	41,186

Notes: Marginal effects dP(Y)/d(X). *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, and round dummies. Weights are 1/(number of observations of that country). Competitiveness variable is calculated from vote margin like -1*(Vote Margin). Larger numbers indicate increasing competitiveness. Proximity is -1*abs(months to/from nearest election), so that larger numbers imply increasing proximity.

Table 4.3.2: Your Living Conditions Compared to Others and Electoral Cycle

Dependent Variable: Your Living Conditions Compared to Others

Electoral Proximity	0.0068***	0.015***	0.014***
	(0.0009)	(0.0093)	(0.0009)
Electoral Competitiveness	- 0.721***	0.378***	0.357***
	(0.08)	(0.029)	(0.0276)
Proximity*Competitiveness	-	0.0178***	0.017***
	-	(0.0011)	(0.0011)
Individual Characteristics	No	No	Yes
Country Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Adj. R^2	0.0677	0.0766	0.1763
N	28,844	28,844	28,844

Notes: Marginal effects dP(Y)/d(X). *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, and round dummies. Weights are 1/(number of observations of that country). Competitiveness variable is calculated from vote margin like -1*(Vote Margin). Larger numbers indicate increasing competitiveness. Proximity is -1*abs(months to/from nearest election), so that larger numbers imply increasing proximity.

Table 4.3.3: Mental Health and Electoral Cycle

Dependent Variable: Mental Health

Electoral Proximity	-0.0017*	0.010***	0.011***
	(0.0009)	(0.0039)	(0.0038)
Electoral Competitiveness	-0.234***	0.212	0.365***
	(0.0812)	(0.159)	(0.154)
Proximity*Competitiveness	-	0.014***	0.018***
	-	(0.004)	(0.0044)
Individual Characteristics	No	No	Yes
Country Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
$Adj.R^2$	0.0492	0.0495	0.1149
N	31,062	31,062	31,062

Notes: Marginal effects dP(Y)/d(X). *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, and round dummies. Weights are 1/(number of observations of that country). Competitiveness variable is calculated from vote margin like -1*(Vote Margin). Larger numbers indicate increasing competitiveness. Proximity is -1*abs(months to/from nearest election), so that larger numbers imply increasing proximity.

Table 4.4: Personal Identification and Subjective Well-Being

Dependent Variable	Your Living Conditions	Your Living Condition Compared to Others	Mental Health
Language/Ethnic/Tribe Group	-0.043**	0.043**	0.108***
	(0.017)	(0.018)	(0.022)
Religion	0.07***	0.149***	0.007
	(0.019)	(0.0209)	(0.025)
Gender	-0.038	0.104***	0.095***
	(0.034)	(0.038)	(0.035)
Other	0.015	0.016	0.058**
	(0.020)	(0.023)	(0.024)
N	20,123	17,554	14,484
$Adj.R^2$	0.1496	0.1178	0.1537

Notes: Occupation/Class is taken as a reference point. Regressions are controlled for individual characteristics, country dummies, and election variables. *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Weights are 1/(number of observations of that country).

Table 4.5: THE SALIENCE OF ETHNIC IDENTIFICATION

Language/Tribe/Ethnic Group	OLS	Logit
Electoral Proximity	0.012***	0.018***
	(0.002)	(0.14)
Electoral Competitiveness	0.63	0.643
	(0.746)	(0.425)
Proximity*Competitiveness	0.010***	0.091***
	(0.0034)	(0.022)
N	20,735	20,735
$Adj.R^2$	0.0742	0.0696

Notes: Marginal effects dP(Y)/d(X). *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies and round dummies. Weights are 1/(number of observations of that country).

Table 4.6.1: Before the election

Dependent Variable	Your Living Conditions	Your Living Condition Compared to Others	Mental Health
Before Election	1.58***	1.44***	1.40***
	(0.105)	(0.248)	(0.172)
Before Election*Electoral Proximity	0.042***	0.044***	0.038***
	(0.003)	(0.0097)	(0.0057)
Before Election*Electoral Competitiveness	1.78***	1.26***	1.37***
	(0.095)	(0.0207)	(0.143)
Before Election*Proximity*Competitiveness	0.051***	0.039***	0.037***
	(0.0043)	(0.012)	(0.0058)
N	41,186	28,844	31,062
$Adj.R^2$	0.1879	0.1723	0.0994

Notes: Marginal effects dP(Y)/d(X). *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, and round dummies. Weights are 1/(number of observations of that country). Competitiveness variable is calculated from vote margin like -1*(Vote Margin). Larger numbers indicate increasing competitiveness. Proximity is -1*abs(months to/from nearest election), so that larger numbers imply increasing proximity. Before the election is 1 if round survey would be before the nearest election.

Table 4.6.2: Public Expenditures

Dependent Variable	Your Living Conditions	Your Condition Compared to Others	Mental Health
PE on Education	0.0758	0.0581	0.0321
	(0.230)	(0.135)	(0.0784)
PE on Health	0.0648*	0.00692	0.00336
	(0.00337)	(0.216)	(0.0744)
PE on Defense	0.0322**	0.0179***	0.0369
	(0.0158)	(0.00885)	(0.181)
N	41,186	28,844	31,062
$Adj.R^2$	0.1821	0.1780	0.1158

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, round dummies, and election variables. Weights are 1/(number of observations of that country).

Table 4.6.3: Winning the election

Dependent Variable	Your Living Conditions	Your Condition Compared to Others	Mental Health
Winning Dummy	0.0401***	0.030**	0.0010
	(0.0108)	(0.0126)	(0.013)
N	41,186	28,844	31,062
$Adj.R^2$	0.1821	0.1780	0.1158

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, round dummies, and election variables. Weights are 1/(number of observations of that country).

Table 4.6.4: Interested in Public Affairs

Dependent Variable	Your Living Conditions	Your Condition Compared to Others	Mental Health
Interested in public affairs	0.0285***	0.0280***	0.015***
	(0.0069)	(0.0082)	(0.0079)
N	41,186	28,844	31,062
$Adj.R^2$	0.1813	0.1765	0.1160

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, round dummies, and election variables. Weights are 1/(number of observations of that country).

Table 4.6.5: Illegitimacy of Elections

Dependent Variable	Your Living Conditions	Your Condition Compared to Others	Mental Health
Trust National Electoral Commission	0.0410***	0.0252***	-0.0057
	(0.00474)	(0.00542)	(0.00592)
N	41,186	28,844	31,062
$Adj.R^2$	0.1799	0.1760	0.1153

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, round dummies, and election variables. Weights are 1/(number of observations of that country).

Table 4.7: Estimates for Individual-level Coefficients

	Your Living Conditions	Your Conditions Compared to Others	Mental Health
Female	-0.00660	0.0198*	- 0.111***
	(0.00963)	(0.0113)	(0.0114)
Age	-0.0198***	-0.00720***	0.00183
	(0.00175)	(0.00197)	(0.00215)
Age^2	0.0170***	0.00734***	- 0.0165***
	(0.00198)	(0.00219)	(0.00247)
Rural	-0.0257**	-0.0776***	-0.0225*
	(0.0107)	(0.0126)	(0.0136)
Informal Schooling	0.0524*	0.143***	-0.0275
	(0.0276)	(0.0320)	(0.0387)
Primary School	0.0778***	0.116***	0.0647***
	(0.0173)	(0.0197)	(0.0219)
Secondary School	0.170***	0.242***	0.161***
	(0.0184)	(0.0213)	(0.0229)
High School	0.310***	0.355***	0.294***
	(0.0235)	(0.0273)	(0.0287)
University	0.400***	0.381***	0.291***
	(0.0283)	(0.0336)	(0.0347)
Post-Graduate	0.492***	0.477***	0.228***
	(0.0672)	(0.0903)	(0.0704)
Employed	0.0694***	0.0811***	0.0289**
	(0.0109)	(0.0130)	(0.0128)
Economic Conditions	-0.217***	-0.267***	- 0.115***
	(0.00537)	(0.00633)	(0.00648)
N	41,186	28,844	31,062
$Adj.R^2$	0.1800	0.1763	0.1158

Notes: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors, clustered at the country level, are reported in parentheses. Regressions are controlled for individual characteristics, country dummies, round dummies, and election variables. Weights are 1/(number of observations of that country).

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